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Essays on Health Insurance and the Family

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Essays on Health Insurance and the Family

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DISSERTATION

Presented to the Faculty of the Graduate School of

The University of Texas at Austin

in Partial Fulfillment

of the Requirements

for the Degree of

DOCTOR OF PHILOSOPHY

THE UNIVERSITY OF TEXAS AT AUSTIN

May 2013

To my parents and my brother. I can't imagine a more supportive family.

Acknowledgments

I am especially grateful to my advisor, Sandra Black, who provided invaluable support and guidance throughout graduate school. I am also thankful for input and feedback from Jason Abrevaya, Marika Cabral, Daniel Hamermesh, Carolyn Heinrich, Gerald Oettinger, Stephen Trejo, and all of the participants in the University of Texas applied microeconomics seminars.

Essays on Health Insurance and the Family

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The University of Texas at Austin, 2013

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The three chapters of this dissertation explore the ties among health insurance, changing cultural institution, and labor economics. The first chapter focuses on the relationship between health insurance and wages by taking advantage of states that extended health insurance dependent coverage to young adults before the Patient Protection and Affordable Care Act. Using American Community Survey and Census data, I find evidence that extending health insurance to young adults raises their wages, both while they are eligible for insurance through their parents' employers and afterwards. The increases in wages can be explained by increases in human capital and increased flexibility in the labor market that comes from people no longer having to rely on their own employers for health insurance.

The second chapter focuses on understanding the impact of allowing coverage of spouses through employer-sponsored health insurance. The fact that people choose to enter into marriage makes comparing the differences between married and unmarried couples uninformative. To get around this, I examine how shocks to access to insurance through a spouse's employer brought on by extensions in legal recognition have influenced health insurance and labor force decisions for same-sex couples. I

find extending legal recognition to same-sex couples results in female same-sex couples being more likely to have one member not in the labor force.

The third chapter examines what extending legal recognition to same-sex couples has done to marriage rates in the United States using a strategy that compares how marriage rates change after legal recognition in states that alter legal recognition versus states that do not. Despite claims that allowing same-sex couples to marry will reduce the marriage rate for opposite-sex couples, I find no evidence that allowing same-sex couples to marry reduces the opposite-sex marriage rate. The opposite-sex marriage rate does decrease, however, when domestic partnerships are available to opposite-sex couples.

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Chapter 1

Do More Health Insurance Options Lead to Higher Wages: Evidence from States Extending Dependent Coverage

Labor market and human capital decisions made by young adults can have lasting impacts on their careers. Despite this, little is currently known about how the need for health insurance coverage affects young adults' labor market decisions. Understanding this is particularly important in light of the fact that extending dependent coverage to young adults is a major component of the Affordable Care Act. Economic theory suggests that having access to employer-sponsored health insurance through a source other than one's own employer could lead to wage increases by reducing job-lock, by allowing people to sort into higher paying jobs that do not offer health insurance, and, as this paper finds, by increasing education. Testing this empirically is difficult, however, because having an alternate source of health insurance, whether it be through a spouse or a parent, is often the outcome of a joint decision. This paper avoids this endogeneity issue by using plausibly exogenous variation in access to a parent's employer-sponsored health insurance plan that is induced by states implementing a minimum age until which employers must provide health insurance to employees' children.

Before the Affordable Care Act required all employers to provide health insurance to employees' children until the age of 26, many states passed reforms that extended dependent coverage to young adults. These reforms gave young adults access to another source of health insurance apart from school or employment and at a price drastically lower than the private market. Although these reforms increased access to employer-sponsored health insurance for young adults, research on the reforms suggests they did not have a dramatic effect on overall health insurance coverage levels. Both Levine et al. (2011) and Monheit et al. (2011) use health insurance data from the Current Population Survey to study how these reforms affected health insurance levels. Levine et al. find overall health insurance rises by about 3 percentage points for young adults, while Monheit et al. find that the main effect of these reforms was to allow young adults to switch from insurance through their own employers to insurance through their parents' employers.

Increased flexibility in the labor market and being able to gain employer-sponsored health insurance through a source other than one's own employer could lead to changes in labor market decisions in a number of ways. First, it could affect education decisions. Attending college at later ages often means people cannot have employer-sponsored health insurance since employers generally allow employees' children to stay on their insurance until the age of 22 at the latest in the absence of the reforms. This makes the opportunity cost of attending college after the age of 22 even higher than the forgone wages since employer-sponsored health insurance is typically cheaper and provides more coverage than individual insurance. Additionally, many colleges require students to have health insurance, which essentially raises the price of college for people without easy access to health insurance. Thus, allowing young adults to stay on their parents' health insurance until later ages could lower both the real and opportunity cost of attending college, which could induce marginal people to attend college and then earn higher wages due to their higher human capital.

Second, having a source of health insurance other than through one's own employer could reduce job-lock, which is the loss of job mobility that arises from the non-portability of employer-sponsored health insurance. As Madrian (1994) argues, with job-lock lessened, people are free to leave their current jobs to find better matches and potentially higher wages. This would be particularly important early in people's careers before people gain experience in careers that are not their best matches.

Finally, compensating differential theory suggests receiving health insurance through a job should lower wages. This suggests extending dependent coverage to young adults would allow them to earn higher wages by sorting into jobs that do not offer health insurance.

This study contributes to the literature along a number of dimensions. First, the results of this paper help us understand what extending health insurance to young adults does and suggest the Affordable Care Act could increase education and wages for young adults. Second, knowing what extending dependent coverage does to education levels helps us understand people's education decisions. Increased college attendance at older ages would suggest the U.S. reliance on employer-sponsored health insurance may prevent people from investing in their human capital.

To determine how this new avenue for obtaining health insurance affects young adults' education and wages, I use data from the Census and the American Community Survey. The estimation strategy compares how education and wages change for eligible young adults after the reforms while accounting for state and national trends. The paper primarily focuses on people older than 22, as younger individuals could generally access parental insurance prior to the change in legislation if they were enrolled in college. I begin by estimating a time-flexible specification that allows the effects of the reforms to vary by an individual's age at the time of the reform to show that the reforms begin to affect people 18 or younger at the time of passage, likely because people 18 and younger have not yet made their higher education and labor force decisions and have not left their parents' health insurance.

I find that wages increase after the age of 22 for those who were 18 or younger when dependent coverage was extended. Wages increase by 2.3 percent for men and 2.9 percent for women when they become eligible for additional health insurance coverage through their parents' employers. These wage increases largely persist beyond the time when young adults are eligible for their parents' health insurance. For men, the persistent changes can be attributed almost entirely to changes in education, which increases by about 0.18 years on average. However, the education gains for women, which are only about 0.06 years and are statistically insignificant, do not seem to account for much of the wage increase. Labor force participation falls slightly for people in their early twenties as men enroll in college and women take more time before entering the labor force. Once young adults are no longer eligible for insurance through their parents' employers, labor force participation returns to the pre-reform levels. Scaling the wage estimates to account for the fact that more employers will have to provide coverage under the Affordable Care Act suggests that the Affordable

Care Act will increase wages by an average of 4.7 to 6.4 percent for people who were 18 or younger when the Affordable Care Act was passed.

The paper unfolds as follows. The next section discusses previous work on health insurance and the labor market. Section 1.2 discusses the extensions in dependent coverage and motivates how health insurance could affect education levels. Section 1.3 discusses the costs of extending dependent coverage. Section 1.4 describes the data, econometric issues, and the empirical strategy. Section 1.5 provides the estimates of the effect of defining dependency status on education levels, education timing, and wages. Section 1.6 provides a discussion of the results, including a possible explanation for why the results might differ for men and women and implications for the Affordable Care Act. Section 1.7 concludes.

1.1 Previous Literature

Employer-sponsored health insurance is cheaper and provides more coverage than individual insurance because of a tax structure that favors employers providing insurance and because risk-pooling is typically easier for employers than for individuals. Furthermore, concerns over adverse selection are a major driving force in the supply-side of the individual market. These factors contribute to the attractiveness of employer-sponsored health insurance relative to alternative sources of health insurance.[1]

A major focus of the literature on health insurance and the labor market is

[1] See Currie and Madrian (1999) and Buntin et al. (2004) for discussions of the advantages that employers have in providing health insurance.

identifying the effects of an outside source of employer-sponsored health insurance on people's labor market decisions and outcomes. Much of this work focuses on married women and uses husbands' insurance coverage to estimate the effect of an outside source of coverage.

Early work identified the effects of an outside source of coverage by treating husbands' health insurance as exogenous in women's labor market decisions.[2] There are two problems with this approach. The first is that the benefits packages of husbands are likely correlated with their unobservable ability and, due to assortative mating, with the unobservable ability of wives. Thus, having an outside option in this case is correlated with an individual's unobserved ability. The second problem, as Currie and Madrian (1999) point out, is that labor force decisions for married men and women may be the outcome of a joint decision, meaning treating one person's health insurance as exogenous may yield inconsistent estimates.

Olson (2002) and Kapinos (2009) deal with assortative mating by instrumenting for a husband's insurance coverage using various characteristics of the husband's job. They find an outside source of insurance coverage raises wages and lowers labor force participation. Although both Olson and Kapinos carefully consider assortative mating, they still make the problematic assumption that couples do not make joint decisions.

This study addresses two key limitations with this literature. The first is one of internal validity in that this paper focuses on an environment in which people

[2]See Buchmueller and Valletta (1999), Holtz-Eakin, et al. (1996), and Lombard (2001) for examples.

making joint decisions is less of a concern since children cannot supply their parents with health insurance. This paper also uses plausibly exogenous variation in the ability to access the outside coverage, meaning the results hold even though parents and children have correlated unobservable traits.

This paper also contributes to the literature by focusing specifically on young adults. Since young adults are at the beginning of their careers, facilitating the job match process may matter more than for other ages and young people may be more likely to invest in their human capital. This is important to know as the United States continues its process of healthcare reform. Young adults being able to use their expanded health insurance options to earn higher lifetime wages indicates the advantages of extending dependent coverage go beyond shifting insurance rates.

1.2 Institutional Details

1.2.1 Defining Dependency Status

Young adults are significantly less likely to be insured than older adults. For the years 2008-2010, 31.1 percent of men and 23.6 percent of women ages 18 to 24 were uninsured, while 18.6 percent of men and 14.7 percent of women ages 25 to 64 were uninsured. This may be due to a number of factors, including the inability to afford health insurance or the decision that it is unnecessary given their age and relative health.[3]

To address the insurance disparities, many states began requiring employers

[3] See Monheit et al. (2011), Nicholson et al. (2009), and Levy (2007) for more thorough discussions of uninsured rates among young adults and their implications.

to allow employees' children to remain on their parents' health insurance plans until later ages. Between 1995 and 2010, thirty-five states formally defined dependency status. Table 1.1 lists the reforms by state. When dependency status is defined, the maximum ages span from 22 to 30.[4]

Table 1.1: Reforms Defining Dependency

State State	Age Limit	Effective Year	State State	Age Limit	Effective Year
Colorado	25	2006	New Hampshire	26	2007
Connecticut	26	2009	New Jersey	30	2006
Delaware	24	2007	New Mexico	25	2003
Florida	25	2007	New York	29	2009
Georgia	25	2006	North Dakota	26	1995
Idaho	25	2007	Ohio	28	2010
Illinois	26	2004	Pennsylvania	30	2010
Indiana	24	2007	Rhode Island	25	2007
Iowa	24	2008	South Carolina	22	2008
Kentucky	25	2008	South Dakota	24	2007
Louisiana	24	2009	Tennessee	24	2008
Maine	25	2007	Texas	25	2004
Maryland	24	2008	Utah	26	1995
Massachusetts	26	2007	Virginia	25	2007
Minnesota	25	2008	Washington	25	2006
Montana	25	2008	West Virginia	25	2007
Nebraska	30	2010	Wisconsin	27	2010
Nevada	24	1995			

Sources: Data on the reforms come from Monheit et al. (2011) as well as data from the National Conference of State Legislatures.

Before the changes in the reforms, the age at which young adults were no longer

[4]Data on the reforms come from Monheit et al. (2011) as well as data from the National Conference of State Legislatures. In most cases, these two sources have identical information on the reforms. When they conflict, I contacted the state insurance department directly or referred to the state legal code.

eligible for health insurance through their parents' employers typically depended on specific employers' policies. Employers traditionally provided coverage for dependents through age 22 if the dependent is enrolled in college and through age 18 otherwise (Government Accountability Office (2008)). States typically require that young adults are unmarried to be eligible for their parents' health insurance since married people can often access insurance through a spouse's employer.[5] [6]

Self-insured employers are covered by federal law due to the 1974 Employee Retirement Income Security Act; therefore, they have no obligation to extend dependent coverage. This is a key difference between these state-level reforms and the Affordable Care Act. In Section 1.6, I compute what the effects of the reforms may be when self-insured employers have to comply as well.

1.2.2 Schooling and Health Insurance

In addition to having employer-sponsored health insurance, young people can often gain coverage from the individual market or through a school insurance plan if they are in college. According to the Government Accountability Office, about half of

[5]Extending health insurance for dependents has the potential to affect marriage decisions if part of the reason that people marry in the absence of the reforms is so they can have health insurance through a spouse's employer. This could affect wages because men tend to experience a marriage premium, while some evidence suggests women experience a marriage penalty. See Ahituv and Lerman (2007) for a thorough summary of this literature as well as some new results. In results not shown, I find no evidence that extending dependent coverage affects marriage decisions.

[6]A few states have other requirements, such as school attendance and residence with parents. Because this paper focuses on the effects for people at older ages, only early-adopting states provide useful variation. Most of these have no financial dependency or college attendance requirement, meaning these results can generally be thought of as applying to states without a financial dependency requirement. All results still hold if all states with stricter requirements are dropped from the sample.

colleges offer student health insurance plans. Despite these other options, a majority of college students have health insurance through their parents' employers, likely because of the cost advantage of this source of coverage. In 2008, the Government Accountability Office found about 67 percent of students aged 18 through 23 were covered through their parents' employer-sponsored plans, while 80 percent of college students had health insurance from any source.

Much research shows the price advantage of employer-sponsored health insurance induces people to participate in the labor force. With young adults, the price advantage could cause marginal people to work instead of attend school. This applies at both the college and high school levels. Thus, once young adults can stay on their parents' health insurance until later ages, the opportunity cost of being in school falls.

Table 1.2: College Tuition and Health Insurance Premiums

	Public Two-Year	Public Four-Year	Private Four-Year
Tuition	\$2,960	\$8,240	\$28,500
		Individual Market, ages 18-24 Average Premium: \$1,320	
Percent of Tuition	45%	16%	5%
		Health Insurance through College Average Premium: \$922	
Percent of Tuition	31%	11%	3%
Percent Requiring Insurance	3%	22%	61%

Notes: The average health insurance premium in the individual market is as of February 2011. The average deductible purchased is around \$3,000. The tuition and the premiums for health insurance through college are for the 2011-12 school year. Sources: College Board, Government Accountability Office, eHealthInsurance report.

Another mechanism for health insurance being related to college attendance is that, as of 2008, 30 percent of colleges required students to have health insurance (Government Accountability Office 2008). This effectively increases the cost of college for anyone without outside coverage, suggesting that extending dependent coverage may effectively reduce the cost of college for people at older ages since people older than 22 cannot be on their parents' plans before the reforms. Table 1.2 shows average tuition cost by type of college as well as the average price of individual plans and student health insurance plans. Two-year schools requiring insurance would impose the highest effective cost increase as a percent of tuition, while private colleges are more likely to require insurance coverage.

1.3 The Cost of Extending Dependent Coverage

Who pays the costs for this increased access to health insurance is unclear. One possibility is that employers do not pass on the costs to families taking advantage of extended dependent coverage. Instead, employers could allow those employees to pay the same amount for health insurance and receive the same wages. This might result in lower profits for the firm or slightly lower wages for all employees. In this case, this health insurance is very cheap for young people relative to other options.

Previous work on mandated coverage, however, suggests employers are successful in passing costs onto the affected population in the form of lower wages. This would mean that parents with adult children would have lower wages after dependent coverage is extended.[7] Alternatively, employers could require parents to pay a higher premium for each additional child on the insurance plan. In both of these cases, the parents would bear the costs. Parents may then require their children to compensate them for the health insurance, meaning extended dependent coverage may not be free for young adults.[8]

It is important to note that extending dependent coverage still has the potential to affect young adults' labor market decisions even if the young adults bear the full cost of the coverage. This is because extending dependent coverage provides young adults with a source of low cost and high benefit coverage without being in the labor force themselves.

This paper estimates the reduced form effect of extending dependent coverage after any cost shifting by employers and parents. Although knowing the exact price changes young adults face would be useful, this is the ideal setting for studying the likely effects of the Affordable Care Act because any cost shifting by firms and parents would presumably happen in similar ways when dependent coverage is available for more young adults under the Affordable Care Act.

[7] See Gruber (1994) for an example of research finding firms pass costs of mandated coverage to employers.

[8] Identifying the effects of extended health insurance on parents' wages is difficult because most eligible children no longer live at home. Using data from the Current Population Survey, I find no evidence that wages or insurance coverage fall for a sample of older adults.

1.4 Data and the Empirical Strategy

1.4.1 Data and Descriptive Statistics

The data on individuals come from the 1990 and 2000 Censuses as well as the 2001-2010 American Community Surveys. Until 2000, the Census asked individuals detailed questions about their demographics and labor market outcomes. Beginning in 2000, the Census Bureau began asking these questions in the American Community Survey instead of the Census. The American Community Survey has smaller sample sizes than the Census but provides yearly data. The advantages of these data sets are that they are large and representative and provide precise estimates of the reduced form effect of extending dependent coverage for young adults.

The estimation uses data on wages, education, and demographic characteristics. I compute the real hourly wage in 2005 dollars by dividing the yearly wage income in 2005 dollars by the product of number of weeks worked and usual hours worked in a week.[9] I then take the log of real wages to use as my dependent variable when I study wages.

When the dependent variable is completed education, the sample will include people over the age of 25 because, as Card (1999) argues, most people reach their ultimate educational attainment by their mid-twenties. Whenever the dependent variable is wages, I consider people over the age of 22 and those between the ages of 18 and 23 separately because I want to compare people at similar points in their careers

[9]The American Community Survey reports number of weeks worked in an interval, so the hourly real wage is obtained by taking the real wage income in 2005 dollars divided by the middle number of weeks in the interval multiplied by the usual hours worked in a week.

Table 1.3: Descriptive Statistics

Men				
	Ages 19 to 22		Ages 23 to 35	
	Mean	Standard deviation	Mean	Standard deviation
Education	12.15	1.48	13.15	2.52
College	0.03	0.17	0.25	0.44
Some College	0.38	0.49	0.50	0.50
High School	0.84	0.36	0.87	0.33
In College	0.41	0.49	0.12	0.32
Age	20.44	1.12	29.08	3.74
Hourly Wage	11.05	101.09	17.79	53.92
Log Hourly Wage	2.13	0.68	2.67	0.68
White	0.74	0.44	0.76	0.43
Black	0.12	0.32	0.10	0.30
Hispanic	0.05	0.22	0.05	0.22
Working	0.63	0.48	0.83	0.38
n	1,266,414		3,943,126	

Women				
	Ages 19 to 22		Ages 23 to 35	
	Mean	Standard deviation	Mean	Standard deviation
Education	12.45	1.46	13.48	2.48
College	0.05	0.22	0.30	0.46
Some College	0.48	0.50	0.58	0.49
High School	0.89	0.31	0.90	0.30
In College	0.51	0.50	0.14	0.35
Age	20.45	1.12	29.10	3.73
Hourly Wage	10.16	21.41	16.18	32.45
Log Hourly Wage	2.04	0.68	2.52	0.69
White	0.74	0.44	0.75	0.43
Black	0.13	0.33	0.12	0.32
Hispanic	0.05	0.22	0.05	0.22
Working	0.60	0.49	0.69	0.46
n	1,216,101		3,948,216	

and because we know people over the age of 22 cannot typically be on their parents' insurance plans in the absence of the reforms, while people under 22 sometimes can be. In order for people at similar ages to be compared to each other, I restrict the data to include only people 35 and younger.[10] The 1990 Census does not ask about current school attendance so the results for education timing draw on only the 2000 Census and the American Community Surveys.

The descriptive statistics are shown in Table 1.3. While the means of the race and age variables are similar for both age groups for men and women, women tend to have more education at all levels, while men tend to have higher wages.

1.4.2 Empirical Strategy

The empirical strategy takes advantage of the facts that the reforms were passed at different times and only affect certain people within states that passed the reforms. The first set of results considers the effects of these reforms on completed education. The empirical strategy can be summarized by the following equation:

$$y_{ist} = \phi_{st} + X_{ist}\alpha + age_{ist}\beta_{1t} + age_{ist}\beta_{2s} + \beta_3 prevtreated_{ist} + \epsilon_{ist}, \tag{1.1}$$

where i indexes individual, t year, s state, y represents completed education, ϕ is a vector of state-year fixed effects, X is a vector of additional controls that includes race, and *age* is a vector of age indicators equal to one for the individual's age and zero for all other ages. The variable *prevtreated*, defined formally below, refers to

[10]Restricting the sample to young people is also important if wages for older people fall because older people are now relatively more expensive to employ.

whether or not the individual was at an age such that he would have been affected by the reform during his early twenties. It will vary for different ages within a state after the reform is passed; thus, β_3 will be the triple-difference estimator of the effect of the reform. Identification comes from comparing how completed education changes for affected ages after the reforms relative to slightly older ages in states that implement the reform relative to those that do not.

Equation (1.1) contains fixed effects for each state and year combination as well as different base levels for each age and state and age and year combinations. Using variation across cohorts within a state and year means that even if states with certain unobservable characteristics implement the reform, consistent estimation is still achieved so long as these characteristics are fixed over time or any changes affect all young adults. The identification strategy is valid as long as characteristics do not change in ways that affect wages and education for people young enough to be affected by the reforms but not people a few years older.

The goal is to define *prevtreated* to be a one if the individual would have gone through his or her early twenties affected by extended dependent coverage and zero otherwise. However, we need to be careful in thinking about who would be affected by these reforms. For instance, if people are 25 years old when legislation is passed that extends health insurance to people up to age 26, they technically have a new health insurance option available to them, but we would expect little effect on these people's education and wages because they have already left their parents' health insurance and made their education and labor force decisions.

I ultimately allow the data to dictate the definition of *prevtreated*. To examine how the effects vary by people's age at the time of reform, I estimate

$$y_{ist} = \phi_{st} + X_{ist}\alpha + age_{ist}\beta_{1t} + age_{ist}\beta_{2s} + \sum_{j \in J} \beta_{3j} age^{rj}_{ist} + \epsilon_{ist}, \tag{1.2}$$

where age^{rj} is a one if the individual was age j when the reform was passed and zero otherwise and J is the set of all ages between 10 and 36 except age 27, meaning all of the coefficients are relative to the effects on people age 27 when the reforms were passed. Age 27 was chosen as the reference age because most of the reforms have an age minimum under 27, meaning there should be no effect for people who were 27 when the reform was passed. Note that $age^r = age - (year - year^r)$, where $year^r$ is the year the reform was passed. We can interpret β_{3j} as being how the reform affects people who were age j at the time of the reform versus people who were age 27 at the time of the reform. Although the β_3 coefficients are too noisy to distinguish among, I graph the coefficients in the next section so we can see how the effects of these reforms vary with age at the time of passage.

Estimating Equation (1.2) will indicate the reforms primarily affect education and wages for people who were 18 or younger at the time of the reform. This is likely because people who were 18 or younger at the time of the reform are likely to have not left their parents' insurance plans or made college or career decisions yet. Because of the results from estimating Equation (1.2), *prevtreated* in Equation (1.1) will be a one if the individual was 18 or younger at the time of the reform but is older than the minimum age set by the reform at the time of the observation, or $prevtreated = 1(age^r \leq 18) * 1(age > agemin)$, where *agemin* is the age minimum for dependent coverage set by the state. This means people at certain ages will have been affected by the reforms while others will not have been.

The results also consider education timing. With education timing, we are no longer interested in whether or not a person was previously treated. Instead, the focus is on people who currently have access to their parents' health insurance. Because people typically have access to their parents' health insurance if they are in college if they are younger than 23, I will distinguish between the effects on people during traditional college ages, or people between the ages of 18 and 23, and the effects on people outside of traditional college ages, or people older than 22. To do this, I include everyone between 18 and 27 in the sample and estimate

$$\begin{aligned} y_{ist} =& \phi_{st} + X_{ist}\alpha + age_{ist}\beta_{1t} + age_{ist}\beta_{2s} + \beta_3 currtreated_{ist} * young_{ist} \\ &+ \beta_4 currtreated_{ist} * older_{ist} + \epsilon_{ist}, \end{aligned} \tag{1.3}$$

currtreated is a one for people who were 18 or younger at the time of the reform and are currently eligible for insurance through their parents' employers, or $currtreated = 1(age_r \leq 18) * 1(age \leq agemin_s)$, *young* is an indicator variable equal to one if the individual is older than 18 and younger than 23, and *older* is an indicator variable equal to one if the individual is older than 22. The coefficient β_3 is the effect of the reform on people during traditional college ages, while the coefficient β_4 is the effect of the reform on people who are older than traditional college ages. We would expect β_3 to be close to zero and β_4 to be positive if extending dependent coverage affects completed education by allowing people to go back to school at later ages.

Equation (1.1) is sufficient for estimating the effect on education because the sample will include only people over the age of 25 and because education is nondecreasing over time, meaning any education increases will persist. When the dependent variable is wages, however, distinguishing between the effects for people who are currently eligible for extended dependent coverage and those who were previously eligible but no longer are is important. Thus, when wages are on the left-hand side, the estimating equation becomes

$$\begin{aligned} y_{ist} =& \phi_{st} + X_{ist}\alpha + age_{ist}\beta_{1t} + age_{ist}\beta_{2s} + \beta_3 currtreated_{ist} \\ &+ \beta_4 prevtreated_{ist} + \epsilon_{ist}. \end{aligned} \tag{1.4}$$

In this equation, β_3 is the effect of the reform on people who are currently eligible for extended dependent coverage, while β_4 is the persistent effect after people no longer have access to their parents' insurance.

1.5 The Effect on Young Adults of Extending Dependent Coverage

1.5.1 Education

To verify that *prevtreated* in Equation (1.1) is defined correctly, I begin by estimating Equation (1.2) with completed education as the dependent variable. The sample includes everyone over the age of 25. Although the results are too noisy to find significant effects for any given year, I graph the β_3 coefficients for men and women in Figures 1 and 2, respectively, to motivate the definition of the treated variable.

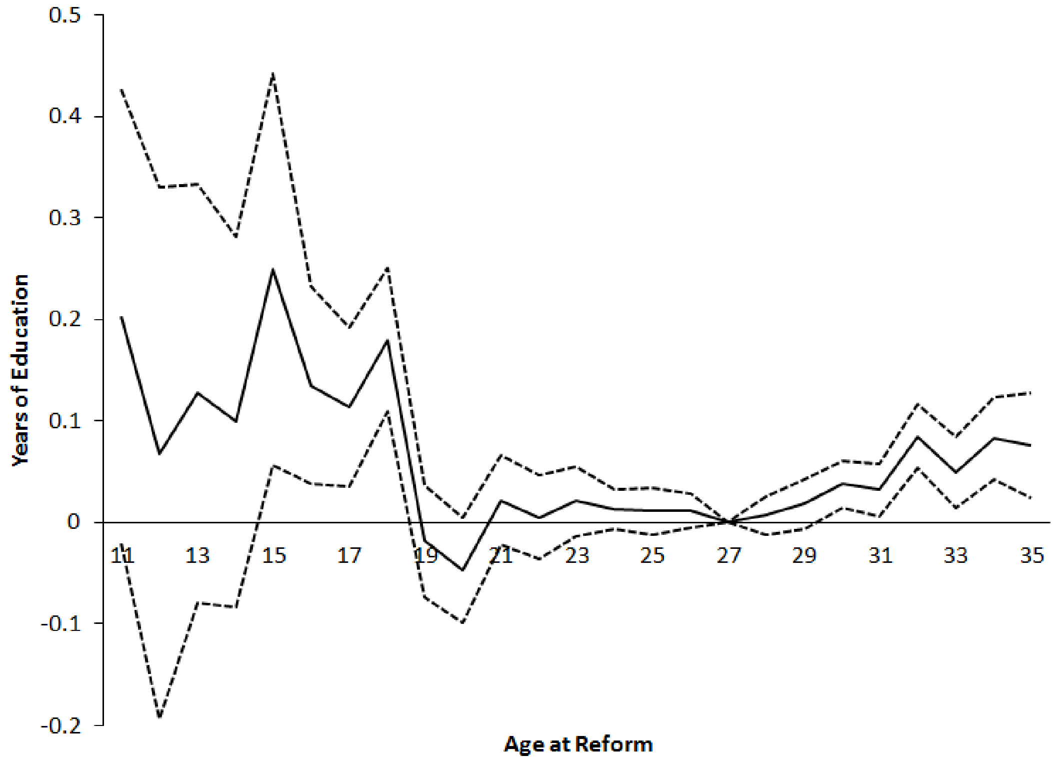

Figure 1.1: This figure shows the coefficients on age at the time of reform from estimating Equation (1.2) for men. The dependent variable is completed education.

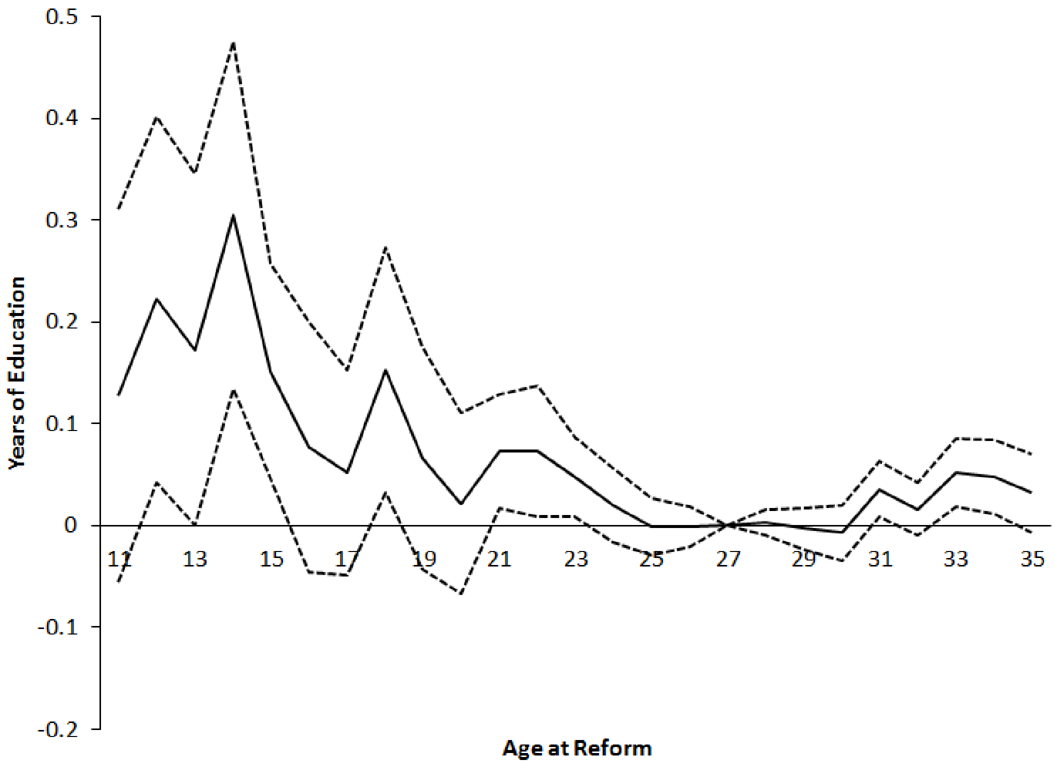

Figure 1.2: This figure shows the coefficients on age at the time of reform from estimating Equation (1.2) for women. The dependent variable is completed education.

From Figure 1, we can see that the reforms had no effect on education for men at older ages when the reforms were passed. Even for people who could technically go back to their parents' insurance, there is little evidence of an effect of the reforms on people's completed education. However, men who were 18 or younger at the time of the reform experience increased education by the time they are older than 25. This suggests the reforms only have effects on education for people who had not yet left their parents' insurance and have not yet made college and labor market decisions at the time of the reform. Figure 2 shows a similar pattern for women although there appears that there might be a small effect on women at slightly older ages. In results not shown, I verify that all of the results hold if a separate variable is included that captures all of the variation coming from people who were ever eligible for insurance through their parents' employers, even if they were the same age as the age requirement when the reforms were passed.

Table 1.4 displays the average effects obtained from estimating Equation (1.1) for men and women with the treated variable equal to a one if the individual is 18 or younger when the reform is passed. In column 1, we see that men who were 18 or younger at the time of the reform have an extra 0.183 years of education on average by the time they are older than 25. In column 2, the dependent variable is a one if people have completed college. The coefficient of 0.025 suggests men are 2.5 percentage points more likely to have completed college by the time they are 26 as a result of dependent coverage being extended. The estimate in column 3 suggests men are 2.6 percentage points more likely have attended some college. In column 4, we can see that there is a smaller but significant effect on the likelihood of completing high school of 1.7 percentage points.

For women the coefficients are generally much smaller and do not show any statistically significant effect except on completing high school. The coefficient in column 1 is 0.055. The coefficients on completing college and completing some college are insignificant and close to zero. The coefficient on completing high school suggests about a one percentage point increase in the number of women graduating from high school.

Table 1.4: The Effect of Extended Health Insurance on Education after Age 25

	Years of education	Completed college	Completed some college	Completed high school
Men				
Previously Treated	0.183***	0.025**	0.026***	0.017***
	(0.061)	(0.009)	(0.006)	(0.006)
Women				
Previously Treated	0.055	-0.006	0.004	0.012*
	(0.062)	(0.008)	(0.018)	(0.007)

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include state-year fixed effects, age indicators interacted with state indicators, age indicators interacted with year indicators, and race controls. There are 3,059,508 observations for men and 3,076,784 observations for women.

1.5.2 Education Timing

The results suggest extending health insurance to young adults increases education, especially for men. As noted earlier, employers typically allow employees' children to stay on their health insurance until they are age 22 if they are in college, meaning we would expect extending dependent coverage to affect people at later ages and not at traditional college ages. I now provide evidence that the people who experienced increased education did so after typical college ages.

Column 1 of Table 1.5 contains estimates of Equation (1.3) with the dependent variable equal to one if the individual is currently attending college and zero otherwise. The sample includes everyone between the ages of 18 and 27.

Table 1.5: The Effect on Labor Force Participation and College Attendance

Men			
	In college	Working	Full-time employment
Currently Treated and Younger than 23	0.001	0.004	0.002
	(0.005)	(0.006)	(0.007)
Currently Treated and Older than 22	0.024***	-0.013***	-0.023**
	(0.007)	(0.004)	(0.009)
n	1,725,711	2,443,532	

Women			
	In college	Working	Full-time employment
Currently Treated and Younger than 23	-0.002	0.005	0.002
	(0.004)	(0.006)	(0.006)
Currently Treated and Older than 22	0.007	-0.012**	-0.007
	(0.007)	(0.005)	(0.010)
n	1,685,340	2,384,018	

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include state-year fixed effects, age indicators interacted with state indicators, age indicators interacted with year indicators, and race controls.

Table 1.6: The Effect on Labor Force Participation and College Attendance for Married People

Men			
	In college	Working	Full-time employment
Currently Treated and Younger than 23	0.001 (0.021)	0.012 (0.011)	0.010 (0.008)
Currently Treated and Older than 22	0.001 (0.013)	0.008 (0.014)	0.003 (0.010)
n	333,036	513,451	
Women			
	In college	Working	Full-time employment
Currently Treated and Younger than 23	0.004 (0.020)	-0.001 (0.014)	0.006 (0.009)
Currently Treated and Older than 22	-0.021 (0.016)	-0.005 (0.009)	0.010 (0.007)
n	482,657	755,361	

The results show that there is no evidence of an effect on college attendance for people 22 and younger. For both men and women, the coefficients are insignificant and close to zero for people younger than 23. Men who were 18 or younger at the time of the reform are 2.4 percentage points more likely to be in college when they are older than 22. This suggests, as expected, extended dependent coverage affects only individuals who were not already covered under their parents' plans. About 15 percent of men were still in college between the ages of 22 and 26, indicating extending dependent coverage increases the number of people still in college between these ages by about 16 percent.

In columns 2 and 3 of Table 1.5, I examine the probability that an individual is working and the probability that the individual has full-time employment. The coefficients for people older than 22 and still eligible for health insurance are negative and significant for both men and women. People are slightly more than one percentage point less likely to be working after the age of 22 while they can still access insurance through their parents' employers. The decrease in full-time employment is even greater for men with a coefficient of -0.023. This suggests men are using the increased access to health insurance to leave the labor force and go back to school. Women are not significantly more likely to be in college, but they may still experience wage increases if they spend more time searching for a job that is a better match and will give them higher wages.

For people to be eligible for insurance through their parents' employers, they have to be unmarried in most states. This means these reforms should have no effect on married people. As a robustness test, I restrict the sample to include only married people and verify that nothing happens to the education and labor force participation decisions of married people after dependent coverage is extended. Table 1.6 reports the results. All of the coefficients are insignificantly different from zero. The only point estimate that appears that it might be different than zero–the coefficient on currently treated and older than 22 for women–is the wrong sign.

1.5.3 Wages

To examine wages, I first focus on everyone over the age of 22 and younger than 36. Column 1 of Table 1.7 contains the results from estimating Equation (1.4). The coefficient on Currently Treated is the impact on wages for people who were 18 or younger at the time of the reform and are currently eligible for insurance through their parents' employers. The estimates suggest wages rise by 2.3 percent for men and by 2.9 percent for women while they are eligible for extended dependent coverage.

The coefficients on Previously Treated test whether or not these effects persist even after young adults are no longer eligible for insurance through their parents' employers. The coefficients of 0.021 and 0.026 for men and women, respectively, indicate that these effects largely persist for both men and women.

The first specification did not control for education because, as was shown earlier, education is endogenous. Column 2 replicates the regressions from column 1 except that it controls for education. The coefficients for people currently treated in column 2 are smaller but still positive and significant for both men and women. For previously treated people, however, the effect for men is almost zero after controlling for education, while the effect for women changes very little. This suggests the

Table 1.7: The Effect of Extended Health Insurance on Wages after Age 22

Men		
Currently Treated	0.023**	0.018**
	(0.009)	(0.008)
Previously Treated	0.021***	0.006
	(0.005)	(0.004)
Education		0.079***
		(0.002)
Women		
Currently Treated	0.029**	0.022*
	(0.013)	(0.012)
Previously Treated	0.026***	0.025***
	(0.008)	(0.004)
Education		0.101***
		(0.001)

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include state-year fixed effects, age indicators interacted with state indicators, age indicators interacted with year indicators, and race controls. There are 3,443,327 observations for men and 3,006,756 observations for women.

increases in education for men explain most of the persistent wage increases, while education changes can account for little of the wage increases for women.

Table 1.8: The Effect of Extended Health Insurance on Wages for People Younger than 23

Men		
Currently Treated	-0.001	-0.001
	(0.009)	(0.009)
Education		0.031***
		(0.002)
Women		
Currently Treated	-0.007	-0.007
	(0.005)	(0.005)
Education		0.044***
		(0.002)

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include state-year fixed effects, age indicators interacted with state indicators, age indicators interacted with year indicators, and race controls. There are 1,028,256 observations for men and 955,075 observations for women.

In Table 1.8, I present the equivalent estimates of Currently Treated for people younger than 23 and find no effects for people at these ages. This could be because the effects of having more insurance options may take time to manifest themselves or because people at these ages often have coverage in the absence of the reforms.

Table 1.9: The Effect of Extended Health Insurance on Labor Force Participation after Age 25

Men		
	Working	Full-time employment
Previously Treated	0.002	0.001
	(0.008)	(0.001)
Women		
	Working	Full-time employment
Previously Treated	0.006	0.007
	(0.005)	(0.007)

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include state-year fixed effects, age indicators interacted with state indicators, age indicators interacted with year indicators, and race controls. There are 3,059,508 observations for men and 3,076,784 observations for women.

All of these estimates are conditional on people being in the labor force. As was shown above, people are less likely to be in the labor force when they are eligible for health insurance through their parents' employers. One possibility is that people exit the labor force because they would have received a low wage, which would cause the average wage of working people to rise even if access to health insurance has no causal impact on wages. Alternatively, people going back to school may be higher ability on average, meaning the estimates may be biased downwards. I next estimate Equation (1.1) with indicators for working and full-time employment as the dependent variables to see if affected people return to the labor force after they are no longer eligible for health insurance through their parents' employers. The coefficients on having been previously treated are shown in Table 1.9. For men and women, the coefficients are close to zero and insignificant. Although people are less likely to work while they have access to their parents' health insurance, they return to the labor force after eligibility at similar levels as people in the state before the reforms. These wage increases persist even after eligibility, though, suggesting selection is not driving the wage increases.

1.6 Discussion

1.6.1 Explaining the Heterogeneous Effects for Men and Women

Both young men and women experience wage increases as a result of having dependent coverage extended to them; however, the underlying mechanisms appear to be different. Education seems to explain part of the wage increases for men but not for women.

One plausible explanation is that women have a higher demand for health insurance. Part of this is due to differences in risk aversion between men and women. Numerous studies have documented that women tend to be more risk averse than men.[11] Another reason women would have a greater demand for health insurance is that they are more likely to have higher costs at this age because they may become pregnant.

This suggests that men would be less likely to have health insurance than women. Using data from the American Community Survey, I graph the rates of uninsurance by age for men and women in Figure 1.3. Men are significantly more likely to be uninsured at all ages. At age 23, where uninsured rates peak for both men and women, women are about 10 percentage points more likely to have health insurance.

[11]For examples, see Jianakoplos and Bernasek (1998), Barber and Odean (2001), Hartog et al. (2002), Agnew et al. (2008), and Borghans et al. (2009).

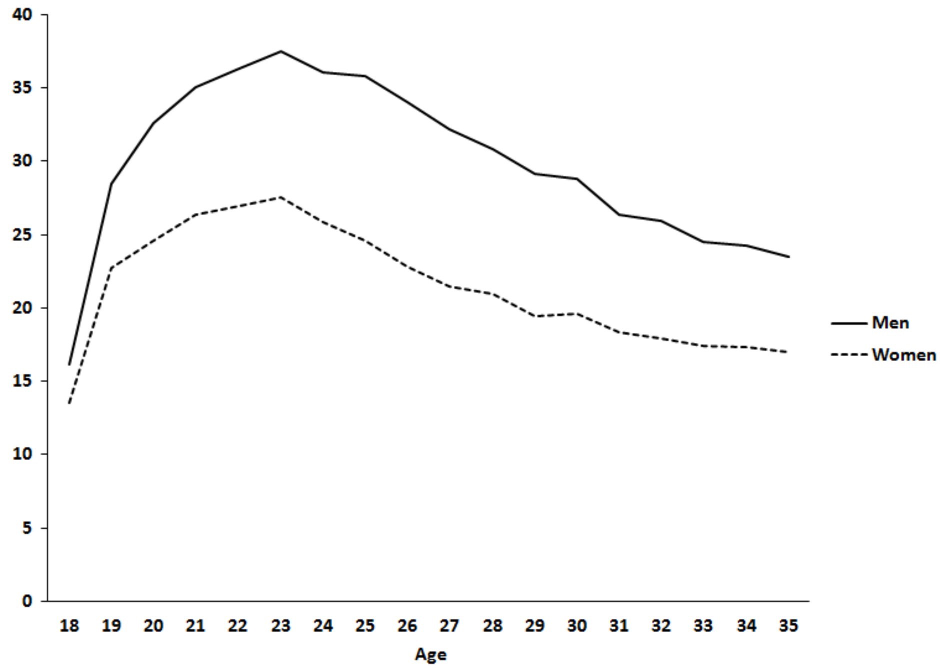

Figure 1.3: Percent Uninsured by Age, 2008-2010

Women's increased desire for health insurance could lead to the wage penalties associated with employer-sponsored health insurance. This suggests that extending health insurance to young women would enable them to move to higher paying jobs that do not offer health insurance. Since they already desire health insurance, requiring them to have health insurance before entering into school does not increase the price of schooling. Men not desiring health insurance as much means job-lock should not be a major driving force in their wages in the absence of extended dependent coverage because being uninsured is not as large a concern; however, since they are less likely to have health insurance, they are more likely to have a higher effective cost of attending college since attending college will require that they purchase health insurance. Providing young men with more options for health insurance lowers this cost.

1.6.2 Implications for the Affordable Care Act

The results show that wages increase for young adults while they are eligible for dependent coverage and that these wage increases persist even after young adults are no longer eligible for insurance through their parents' employers. I now calculate what the estimates in this paper suggest will be the effect of the Affordable Care Act.

Table 1.10: Predicting the Wage Effects of the Affordable Care Act

	Effects on Currently Treated	
	Effect on Log Wages	Predicted Effect of the Affordable Care Act
Men	0.023 (0.009)	0.051
Women	0.029 (0.013)	0.064
	Persistent Effects	
	Effect on Log Wages	Predicted Effect of the Affordable Care Act
Men	0.021 (0.005)	0.047
Women	0.026 (0.008)	0.058

The Affordable Care Act requires employers to allow employees' children to stay on their insurance until age 26. There are two main differences between this provision of the Affordable Care Act and the state-level reforms. The first is that everyone will be eligible to be on their parents' health insurance plan under the Affordable Care Act. Almost all of the reforms discussed in this paper require that people be unmarried. We would expect the Affordable Care Act to have smaller effects for married people since many married people have access to health insurance through their spouse's employer, meaning they already have access to employer-sponsored health insurance through a source other than their own employer.[12] I compute the predicted effect of the Affordable Care Act assuming no effect on married couples, meaning the results here are a lower bound on the impact of the Affordable Care Act.

The second difference between the state-level reforms and the Affordable Care Act is that self-insured employers will also have to comply with the Affordable Care Act. This will allow more people to receive health insurance through their parents' employers and will thus result in a higher average wage increase. Recent estimates suggest that 55 percent of people with employer-sponsored health insurance have it through self-insuring employers (Employee Benefit Research Institute (2009)). This number implies the estimates should be scaled up by a factor of 2.2 since more people will have access to dependent coverage.[13]

Table 1.10 displays the predictions for wages. The estimates presented earlier suggest that men experience wage increases of 2.3 percent and women experience wage

[12]Data from the CPS indicates about 28 percent of people age 23 to 26 were married in 2010 and that about half of young married couples take advantage of the ability to gain insurance through a spouse's employer.

[13]Another difference between these state-level reforms and the Affordable Care Act is that the Affordable Care Act has an age minimum of 26, while the age minimums for the state reforms vary. Allowing the estimates of the state-level reforms to vary by the number of years of additional coverage produces very similar predictions for the Affordable Care Act.

increases of 2.9 percent while they have access to their parents' health insurance. When self-insured employers have to comply, we might expect effect sizes of 5.1 percent for men and 6.4 percent for women, which would be over a $9,000,000 increase in total earnings in 2010 dollars for people ages 23 to 26. The estimates of the persistent effects from earlier suggest the affected cohort of men has 2.1 percent higher wages, while the affected cohort of women has 2.6 percent higher wages. Scaling the estimates for the Affordable Care Act suggests wages will increase by 4.7 percent for men 5.8 percent for women.[14]

1.7 Conclusion

Understanding what happens when people do not need to be in the labor force to have access to health insurance is important as the United States continues its process of healthcare reform. Although economic theory suggests a number of ways people could earn higher wages, testing this is difficult since people typically only have outside coverage if they are married. Likely because of the empirical challenges, no studies to date have examined the effect of a low-cost outside source of health insurance on wages for young people, an important and unique group.

This paper has shown that having an outside source of employer-sponsored health insurance when young leads to increased wages, both when people have in-

[14]One caveat to this prediction is that determining how long these wage increases will last is difficult since we cannot track people very far into their adult lives. We would expect the wage increases coming from education to persist, but wage increases coming from improved job matches might not last if these people would have eventually found better matched jobs without extended dependent coverage.

creased access to health insurance and afterwards. For men 18 and younger when dependent coverage was extended, wages increase by 2.1 percent after they are no longer eligible for insurance through their parents' employers, while wages increase by 2.6 percent for women 18 and younger at the time of the reform once they are older than their state's minimum age.

For men, wages increase because education increases by an average of 0.18 years. These increases in education arise because extending dependent coverage lowers the cost to being in school at later ages. The opportunity cost of being in school falls since being in school no longer means losing access to cheap and generous health insurance. The real cost of being in school also falls for many people since some colleges require students to have health insurance, which raises the effective price of attending college for people who do not desire health insurance.

The estimates from this paper suggest the Affordable Care Act will increase wages for young adults by an average of 4.7 to 6.4 percent. Knowing that the Affordable Care Act has benefits that go beyond providing more coverage is important in understanding the potential impact of this legislation.

The paper also finds suggestive evidence that the early effects of the Affordable Care Act are consistent with the results found from the state-level reforms that extended dependent coverage will allow people to be in school at later ages.

Chapter 2

Health Insurance and the Labor Force: What Legal Recognition Does for Same-Sex Couples

Married couples in the United States have different labor force participation rates than unmarried couples. According to 2000 to 2011 CPS data, 27 percent of married couples have one member in the labor force and one not in the labor force, while this is true for only 22 percent of unmarried opposite-sex couples. Married couples are also much more likely to be able to receive insurance through a spouse's employer. Over 65 percent of married couples take advantage of the ability to receive insurance through a spouse's employer, while only 7 percent of unmarried couples report one member having health insurance through the other's employer. The ability to receive health insurance through a spouse's employer could allow couples more flexibility in the labor market, but the difference in labor force participation rates could also reflect that a certain type of couple selects into marriage. Since couples choose whether or not to get married, we cannot simply compare married couples to unmarried couples. In this paper, I study how labor force participation and health insurance coverage change for same-sex couples after they can enter into legal recognition through marriage, civil unions, and domestic partnerships.

Although for opposite-sex couples marriage has many economic benefits, most of the economic benefits of marriage are still denied to same-sex couples due to the Defense of Marriage Act, a federal law that limits federal recognition of same-sex couples. However, many same-sex couples have had their health insurance options change because these new forms of legal recognition mean that most employers who offer health insurance to opposite-sex spouses have to offer health insurance to same-sex spouses as well. Thus, allowing same-sex couples to enter into legal recognition can affect their labor force participation because now only one spouse needs to be working full-time to provide both members of the couple with health insurance.

To examine empirically how same-sex couples respond to legal recognition, I use data from the March Current Population Survey from 1996 to 2011 to implement a triple-difference estimation strategy. The first difference compares how labor force participation and health insurance coverage change after states extend legal recognition. This accounts for initial state differences. The second difference is how these variables have changed over time in states that do not extend legal recognition. Comparing how labor force participation and health insurance change in states that don't extend legal recognition is important because some companies have begun to offer health insurance benefits to same-sex partners even when not required to do so by state governments. This might result in a false rejection of the null hypothesis of no effect if I simply compared same-sex couples before and after legal recognition. The third difference is how these two differences change between same-sex couples and married opposite-sex couples. Using married opposite-sex couples as a within-state control group is important because many states have instituted policies aimed at increasing insurance coverage for everybody as the uninsured population has become a bigger policy focus.

Because properly identifying same-sex couples can itself be a challenging task, I pay special attention to the data and take advantage of a recently added question in the CPS to help identify same-sex couples. The treatment group is all cohabiting same-sex couples instead of only those who choose to enter into marriage or other legal unions. This is required because of the structure of the data, but it also helps avoid endogeneity issues associated with which same-sex couples choose to enter into legal recognition. I focus on couples where both members are between the ages of 30 and 65 because people older than 65 are eligible for Medicare and because many states and the federal government passed legislation that extends dependent coverage to young adults through their parents' employers.

I find evidence that legal recognition affects labor force participation and health insurance coverage for women but not men. After legal recognition, women in same-sex couples experience a decrease in labor force participation, an increase in health insurance through their spouse's employer, and an equally offsetting decrease in insurance through their own employer. The differences in how men and women react to the changes in legal recognition can partly be explained by the fact that about one-third of female same-sex couples are raising young children while almost no male same-sex couples are, as female same-sex couples with young children exit the labor force at a much higher rate after legal recognition. This suggests that women in same-sex couples would prefer for one member of the couple to devote herself more fully to parenting but are prevented from doing so in the absence of legal recognition. One of the main arguments used against allowing same-sex couples to marry has been that opposite-sex parents are ideal for raising children. However, it seems allowing same-sex couples to marry and to enter into other forms of legal recognition actually allows same-sex couples to specialize more and devote more time to raising their children.

The paper proceeds as follows. Section 1 describes the relevant literatures on health insurance and the labor market. Section 2 discusses theoretical predictions for how legal recognition might affect health insurance and labor force participation for same-sex couples. Section 3 discusses the various forms of legal recognition for same-sex couples and how they differ from each other and marriage for opposite-sex couples. Section 4 discusses the data and the identification strategy. Section 5 presents the results. Section 6 considers the robustness of the results to various data choices as well as issues of migration and control group selection. Finally, section 7 concludes.

2.1 Previous Literature

Empirical challenges arise in estimating the impact of health insurance on labor force participation because labor market and health insurance decisions are made jointly. Previous research on labor force participation and health insurance has tried circumventing this issue by focusing on married women's labor force participation and treating husbands' employer-sponsored health insurance as exogenous. Under this assumption, Olson (1997) estimates that women with husbands without employer-sponsored health insurance have a 7-9 percent higher level of participation in the labor force than women with husbands with employer-sponsored health insurance, while

Buchmueller and Valletta (1999) estimate a 6-12 percent higher level of labor force participation for women with husbands without employer-sponsored health insurance.

As Currie and Madrian (1999) point out with labor force participation, the assumption made to identify these effects is problematic if husbands and wives make joint labor supply decisions. This paper skirts the empirical problems that have hindered previous research by focusing on an environment in which access to health insurance through a spouse's employer is exogenously changed and examines how this affects labor force decisions and overall coverage levels for couples.

One recent paper studies how same-sex couples respond to the option of receiving health insurance through a spouse's employer. Buchmueller and Carpenter (2012) use California Health Interview Surveys to study how health insurance and labor force participation change for gays and lesbians after California began requiring private employers to extend employer-sponsored coverage to same-sex spouses if they extend it to opposite-sex couples. They find that lesbians are more likely to have insurance through a spouse's employer and are less likely to work full time. The current paper departs from their analysis by considering all states that have provided legal recognition. Additionally, CPS data provide information about both members of a couple, which allows for directly testing theories about how legal recognition would affect within couple changes, which is an important outcome of interest. Unlike Buchmueller and Carpenter (2012), this paper also compares how outcomes for same-sex couples have changed in non-treated states because a lot has changed for same-sex couples nationally.

2.2 The Conceptual Framework

In the absence of legal recognition of same-sex couples, employers are not required to offer health insurance to same-sex partners even if they offer it to opposite-sex spouses. Legal recognition has the potential to affect labor force participation because now only one member of the couple needs to be in the labor force to provide both members with employer-sponsored health insurance. This means that the average labor force participation for people in same-sex couples may go down because the number of couples with both members in the labor force can fall.[1]

Couples taking advantage of the ability to receive health insurance through a spouse's employer would mean that the number of people in same-sex couples receiving insurance through a spouse's employer should rise after legal recognition. This would mean that people either switch from insurance through another source or that overall coverage levels rise. If people switch from another source, legal recognition will result in decreases in the number of people with insurance through their own employers, through privately purchased health insurance, or through Medicaid. If couples exit the labor force because of the new insurance options available to them, we would expect insurance through one's own employer to fall.

[1] Alternatively, one member of the couple can enter into the labor force to provide both members with health insurance if neither was working before recognition. This could cause average labor force participation to rise. The number of couples with neither member in the labor force is small, and there appears to be no evidence that people are induced to participate in the labor force after legal recognition.

2.3 Legal Recognition for Same-Sex Couples

The first legal challenge for recognition of same-sex couples in the United States came in Hawaii in 1993 (Sullivan (2008)). In Baehr v. Lewin, the Hawaii Supreme Court ruled that refusing to issue marriage licenses to same-sex couples was discrimination on the basis of sex. The court argued that under Hawaii's Equal Rights Amendment, the state would have to establish a compelling state interest to continue the ban and remanded the case to a lower court to decide if this standard could be met. In response to this ruling, laws against same-sex marriage were passed by many states and by the federal government. The 1996 Defense of Marriage Act defines marriage as a union between a man and a woman so no states would be forced to legally recognize same-sex couples. The Defense of Marriage Act also denies federal marriage benefits to same-sex couples.

States have named the legal recognition for couples domestic partnerships, civil unions, or marriage. The specifics of the laws vary by state, but they are all designed to provide similar state-level protections as marriage, meaning the typical rights tend to be similar. For this reason, I treat all three names for legal recognition as being the same.[2] In many states, the laws explicitly say that employers are required to provide health insurance to same-sex spouses if they provide it to opposite-sex spouses, while in other states the law says same-sex couples are to be treated the same as opposite-sex couples, meaning there can be no discrimination in providing health insurance

[2] In addition to the ability to gain access to insurance through a spouse's employer, common rights covered include hospital visitation rights, family leave for a sick or dying partner, the right for partners to share a nursing home room, the ability to inherit a partner's estate in the absence of a will, and immunity from testifying against a partner.

Table 2.1: States Extending Legal Recognition

State	Same-Sex Marriage Date	Civil Union Date	Domestic Partnership Date
California			9/22/1999 L
District of Columbia	3/9/2010 L		1/1/2002 L
Colorado			7/1/2009 L
Connecticut	11/12/2008 J	10/1/2005 L	
Hawaii		1/1/2012 L	
Illinois		6/1/2011 L	
Iowa	4/2/2009 J		
Maine			7/30/2004 L
Maryland			7/1/2008 L
Massachusetts	5/17/2004 J		
Nevada			10/1/2009 L
New Hampshire	1/1/2010 L	1/1/2008 L	
New Jersey		2/19/2007 J	7/10/2004 L
New York	7/24/2011 L		
Oregon			2/4/2008 L
Vermont	9/1/2009 L	7/1/2000 J	
Washington			7/22/2007 L
Wisconsin			8/3/2009 L

J indicates that the law came about through the judicial system, while L indicates that it was passed by a state legislature.
Laws passed as of March 2012.

to spouses. Although same-sex couples still have to pay federal income taxes on employer-sponsored health insurance for a spouse, they do not have to pay any state taxes on these benefits. Employers that self-insure are covered by federal law and as such have no obligation to offer health insurance to same-sex spouses.[3] [4]

As of March 2011, fifteen states and the District of Columbia provide some form of legal recognition for same-sex couples. In all states, any form of legal recognition has come about through either state Supreme Court rulings or action by the state legislature. Until 2012, popular votes on legal recognition had almost always gone against same-sex couples. Thirty states have altered their Constitutions to ban same-sex marriage.[5] Table 2.1 lists the laws by state.

Eligibility rules for entering into these unions vary by state. In states with same-sex marriage, the rules are typically the same for same-sex spouses and opposite-sex spouses. In most states with domestic partnerships and civil unions, the couple needs to live together, and both members of the couple need to be at least 18. In many of the states, domestic partnerships and civil unions are only available to same-sex couples, but some states extend this option to opposite-sex couples as well.[6]

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[3]See Employee Benefit Research Institute (2009) for a more thorough discussion of employers' health insurance obligations when states allow same-sex couples to enter into legal recognition.

[4]According to the Government Accountability Office, the Defense of Marriage Act denies 1,138 federal rights to same-sex couples. For a complete listing of the rights denied to same-sex couples by the Defense of Marriage Act, see the full report available on the Government Accountability Office's website, http://www.gao.gov/new.items/d04353r.pdf.

[5]Two states–California and Maine–reversed same-sex marriage after it was legalized. These are the only instances of states removing legal recognition after it had been granted. Both states continue to offer domestic partnerships.

[6]Studies by the Williams Institute have found that 50 percent of same-sex couples tend to marry within the first few years and use this as a starting point in forming their estimates of the impact of marriage on state economies. See Badgett et al. (2007) for example.

2.4 Data and the Empirical Strategy

2.4.1 Data and Descriptive Statistics

The data come from the 1996 to 2011 March Current Population Surveys. The March CPS provides annual data that include demographic characteristics, health insurance coverage, and labor force participation.[7] Not only does the March CPS provide information about whether or not individuals have health insurance, we also know the source of this coverage. I consider four different sources of insurance: 1) health insurance through one's own employer 2) health insurance through a spouse or partner's employer 3) privately purchased health insurance and 4) public health insurance (Medicaid and military or veteran's insurance). I also consider a variable equal to 1 if people have health insurance coverage and 0 otherwise.

Starting in 2007, the CPS began explicitly asking people if they had a partner in the household; for these years, I include couples identified using this question. Before 2007, I take advantage of the fact that we know everyone's relationship to the head of the household, which makes it possible to identify same-sex couples since anyone in the household can be classified as being an unmarried partner of the head of the household. Everyone in the sample is a head of household, a spouse of the head of the household, or an unmarried partner of the head of the household before 2007. Two people are identified as being in a same-sex couple if one person is coded as being an unmarried partner of a head of the household of the same sex.[8]

[7]The CPS asks about health insurance during the previous year, so I create my labor force participation variable by coding anybody who worked in the previous year as having participated in the labor force. Results are similar if I use current labor force participation.

[8]Black et al. (2000) consider Census data in detail and find that this approach to identifying

Table 2.2: Means of Key Variables for Same-Sex Couples in 2006 and 2007

	2006	2007
Labor force participation	0.90	0.88
	(0.02)	(0.01)
Both members in labor force	0.82	0.79
	(0.02)	(0.02)
One member in labor force	0.15	0.17
	(0.02)	(0.02)
Neither member in labor force	0.03	0.03
	(0.01)	(0.01)
Any health insurance	0.84	0.79
	(0.02)	(0.02)
Insurance through spouse's employer	0.09	0.07
	(0.02)	(0.01)
Insurance through own employer	0.64	0.63
	(0.03)	(0.02)
Privately purchased insurance	0.09	0.05
	(0.02)	(0.01)
Public insurance	0.08	0.08
	(0.02)	(0.01)
Age	40.18	39.39
	(0.6)	(0.44)
Education	13.13	12.88
	(0.17)	(0.13)
White	0.82	0.85
	(0.02)	(0.02)
Black	0.08	0.07
	(0.02)	(0.01)
Hispanic	0.14	0.16
	(0.02)	(0.02)
Children younger than 18	0.32	0.24
	(0.03)	(0.02)
Children younger than 5	0.12	0.10
	(0.02)	(0.01)
n	310	526

The CPS question that explicitly asks about partnership status allows us not only to identify more same-sex couples but possibly different types of couples as well because starting in 2007, we can identify same-sex couples when neither member is the head of the household. To see if there are major differences in the types of couples identified with the different methods, Table 2.2 compares same-sex couples in 2006 and 2007. In 2007, we can identify approximately 70 percent more same-sex couples; however, the characteristics of the members of the same-sex couples are similar in both years.

In 2010 and 2011, when two people of the same sex report being married, the CPS changes the relationship status so that the married same-sex couple shows up as an unmarried same-sex couple. A concern arises with this coding procedure because there are many more opposite-sex couples in the sample than same-sex couples. Although almost no one misreports his or her gender, even if only a small percentage of people in opposite-sex couples do misreport, the sample of people identified as being in same-sex couples might consist of a high percentage of people in misreporting opposite-sex couples. In an attempt to keep the control and treatment groups pure, I drop any couple from the sample when either member has had his or her sex or marital status changed.

Before 2010 when two people of the same sex report being married, the CPS changes the sex of the spouse so that the couple shows up in the data as a married

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same-sex couples is generally accurate. Ash and Badgett (2006), who use CPS data, use the same method and compare their sample to samples found using the Census and find that the samples from the different sources have very similar characteristics.

opposite-sex couple. This is problematic for the estimation because beginning in 2004, same-sex couples can marry. When they marry, they are included in the sample as married opposite-sex couples. This type of measurement error will bias all of the estimated effects of legal recognition on same-sex couples towards zero. This data issue is mitigated by the fact that only one state, Massachusetts, allows same-sex couples to marry before 2007 when the CPS added the new question to help identify same-sex couples. A potential concern is that reporting being married is also related to the passage of non-marriage legal recognition laws. This would occur if couples considered themselves to be married after they entered into non-marriage recognition and would result in the treated subsample being dropped.

To see how legal recognition and the CPS coding procedures affect the likelihood of identifying same-sex couples, I estimate the following linear probability model:

$$y_{ist} = \phi_t + v_s + \beta_1 marriage_{st} + \beta_2 altreg_{st} + \epsilon_{ist}, \tag{2.1}$$

where i indexes the couple, s indexes the state, t indexes the year, y is an indicator for whether or not the couple is of the same sex, ϕ is a vector of year effects, v is a vector of state effects, $marriage$ is an indicator for the state allowing same-sex couples the right to marry, $altreg$ is an indicator for the state allowing same-sex couples to enter into alternate recognition, and ϵ is a couple-specific random error term.

The results are shown in Table 2.3 and show how the changes in legal recognition are associated with changes in the chances of identifying a same-sex couple. Column 1 reports coefficients for before 2007, and column 2 reports coefficients for

Table 2.3: Effects of Legal Recognition on Identifying Same-Sex Couples

	Before 2007	2007 and Beyond
Marriage	-0.0037***	-0.0004
	(0.0003)	(0.0018)
Alternate recognition	0.0019	-0.0002
	(0.0012)	(0.0014)
n	353,072	189,255

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. The specifications control for *state* and *year*.

2007 and beyond. For reference, close to 1 percent of the couples are identified as being of the same sex. Although it is possible to think of stories for how legal recognition could change the numbers of same-sex couples identified in the sample, the coefficient on *marriage* of -0.0037 in column 1 raises concern. It appears that once same-sex couples in Massachusetts can marry, they report they are married and are counted as opposite-sex couples. This problem seems to be fixed once the new question is asked about having a partner in the household. Because I do not want to contaminate the treatment and control groups, I drop couples from Massachusetts during the few years that same-sex couples can marry before 2007 from the sample. After the CPS begins explicitly asking about partners in the household, marriage is no longer associated with a decrease in the likelihood of identifying a same-sex couple.

These results also allow us to consider if there is a selection issue. The value of being in a cohabiting same-sex relationship could also change with the introduction of legal recognition, which could result in more or different types of people living together, especially since cohabitation is a requirement to enter into the new forms of legal recognition in many cases. We see no evidence of jumps in the likelihood of identifying couples after changes in legal recognition under either coding method by the CPS.

The sample includes only people older than 30 because many states and the federal government have adopted policies during the time period studied that extend dependent coverage through a parent to young adults.[9] The sample includes only people younger than 65 because people 65 and older are eligible for Medicare.

Table 2.4 displays the descriptive statistics. The sample contains 1,708 women in same-sex couples and 1,690 men in same-sex couples. Men in same-sex relationships tend to be younger than married men in opposite-sex couples and older than men in unmarried cohabiting opposite-sex relationships. They also have a higher mean education and are more likely to be white. Men in same-sex couples are slightly less likely to be in the labor force than men in married opposite-sex relationships. Men in same-sex couples are more likely to have health insurance through their own employers than anyone else in the sample. They are more likely to have health insurance through a spouse or partner than unmarried men in opposite-sex relationships and less likely to have health insurance through a spouse or partner than married men in opposite-sex couples.

As with men, women in same-sex couples tend to be older than unmarried

[9] The effects are smaller if people in their twenties are included in the sample. Providing legal recognition to same-sex couples appears to have little to no effect on people in their twenties, likely because young couples have other sources of coverage available to them or because they are still completing their educations or not entering into marriage.

Table 2.4: Means of Key Variables

	Women in married opposite-sex couples	Women in same-sex couples	Women in unmarried opposite-sex couples
Labor force participation	0.75	0.85	0.81
Both members in labor force	0.70	0.75	0.73
One member in labor force	0.26	0.20	0.22
Neither member in labor force	0.04	0.04	0.05
Any health insurance	0.89	0.84	0.73
Insurance through spouse's employer	0.50	0.10	0.04
Insurance through own employer	0.37	0.64	0.51
Privately purchased insurance	0.07	0.07	0.06
Public insurance	0.09	0.10	0.14
Age	44.68	44.69	42.58
Education	11.98	13.20	11.04
White	0.86	0.88	0.81
Black	0.07	0.07	0.12
Hispanic	0.12	0.13	0.14
Children younger than 18	0.71	0.35	0.52
Children younger than 5	0.17	0.11	0.11
n	404,168	1,708	22,477
	Men in married opposite-sex couples	**Men in same-sex couples**	**Men in unmarried opposite-sex couples**
Labor force participation	0.91	0.89	0.87
Both members in labor force	0.70	0.81	0.73
One member in labor force	0.26	0.16	0.22
Neither member in labor force	0.04	0.03	0.05
Any health insurance	0.89	0.86	0.69
Insurance through spouse's employer	0.24	0.08	0.03
Insurance through own employer	0.63	0.69	0.53
Privately purchased insurance	0.08	0.09	0.07
Public insurance	0.09	0.07	0.11
Age	46.75	44.61	44.33
Education	12.01	13.70	10.77
White	0.87	0.86	0.80
Black	0.07	0.07	0.14
Hispanic	0.12	0.13	0.14
Children younger than 18	0.71	0.09	0.52
Children younger than 5	0.17	0.03	0.11
n	404,168	1,690	22,477

women in opposite-sex couples and younger than women in married opposite-sex couples. Women in same-sex couples have more education than either of the other groups. Women in same-sex couples are about 10 percentage points more likely to be in the labor force than married women in opposite-sex couples. Women in same-sex couples are less likely to have health insurance than women in unmarried opposite-sex couples and more likely to have it than women in unmarried opposite-sex couples.

About one-third of female same-sex couples have children living in the household, and about 11 percent have a child younger than five in the household. The percentage of male same-sex couples with children is much smaller. Although the number of same-sex couples adopting has risen dramatically over the past decade, most of the same-sex couples with children have them from a previous relationship with someone of the opposite-sex (Black et al. (2000)).

2.4.2 Empirical Strategy

The goal is to determine if and how extending legal recognition to same-sex couples has affected their health insurance coverage and their labor force participation. One potential concern is that some companies have begun to offer spousal health insurance benefits to same-sex couples even when they are not required to do so. A failure to account for this in the estimation strategy could bias the effects of extending legal recognition to same-sex couples. Another possible problem is that various states have changed their insurance policies and systems over the years in an effort to help people attain health insurance and deal with rising costs of medical care. This could lead the effects of health insurance reform to be attributed to the effects of legal recognition if we did not include a within state control group. To ensure the estimation strategy does not confound these factors with the true effects of legal recognition, I compare same-sex couples to married opposite-sex couples before and after the passage of the laws in states that change their laws versus those that do not. In addition, I account for time trends specific to same-sex couples. Because some of these laws apply to unmarried opposite-sex couples as well, I separate out the effects for unmarried opposite-sex couples and married opposite-sex couples. This empirical strategy is equivalent to a triple-difference estimation strategy where we have treatment states being those that extend legal recognition, a treatment group and a control group being same-sex couples and married opposite-sex couples, respectively, and a pre and post period being before and after the passage of the laws.

The basic estimating equation for how these laws have affected couples is

$$\begin{aligned} y_{ist} =& \phi_{st} + X_{ist}\alpha + cohabit_{ist}\beta_{1t} + cohabit_{ist}\beta_{2s} + \beta_3 L_{st} * cohabit_{ist} \\ &+ samesex_{ist}\beta_{4t} + +samesex_{ist}\beta_{5t} + \beta_6 L_{st} * samesex_{ist} + \epsilon_{ist}, \end{aligned} \tag{2.2}$$

where i indexes either the couple or the individual, s indexes the state, t indexes the year, y represents the various dependent variables used, ϕ is a vector of state-year fixed effects, X is the vector of additional controls, including race, education, and age, *cohabit* is an indicator for whether or not an individual is in an unmarried opposite-sex cohabiting relationship, *samesex* is an indicator for whether or not an individual is a member of a same-sex couple, L is an indicator for whether or not the state currently offers legal recognition to same-sex couples, and ϵ is an individual-specific random error term. The β_{4t} coefficients allow the effect of being in a same-sex couple to vary over time, and the β_{5s} coefficients allows states that offer legal recognition to have different baselines for people in same-sex couples than states that do not. The β_{1t} and β_{2s} coefficients do the equivalent for unmarried opposite-sex couples. The coefficient β_3 is the effect of legal recognition on opposite-sex couples, and the coefficient β_6 is the effect of legal recognition on same-sex couples.

The main dependent variables considered are labor force participation and the various forms of health insurance. For the health insurance variables, the dependent variables are binary indicators for whether or not the individual has a certain type of health insurance. To examine labor force participation, I first estimate a linear probability model as in Equation (2.2) with labor force participation coded as a binary variable. This gives us the average effect of legal recognition on labor force participation, but the main effect of changes in health insurance actually may be to increase specialization within a couple. To examine this, I consider the joint decisions of the couples by estimating separate couple-level specifications where the dependent variable is equal to 1 if both members are in the labor force and 0 otherwise, a 1 if one member is in the labor force and the other is not and 0 otherwise, and a 1 if neither member is in the labor force and 0 otherwise. When estimating models where the unit of observation is the couple instead of the individual, I estimate models similar to Equation (2.2) with the only difference being that the demographic characteristics of both members of the couple are included.

The coefficient β_6 is identified by comparing same-sex couples to married couples. It is not immediately clear who to use as the control group–married opposite-sex couples or unmarried opposite-sex couples. In states with legal recognition available, some same-sex couples enter into legal unions, making them more like married opposite-sex couples, while other same-sex couples do not enter into the legal unions, making them more like unmarried opposite-sex couples. The problem with comparing same-sex couples to unmarried opposite-sex couples is that in some states the new forms of legal recognition are extended to unmarried opposite-sex couples as well as same-sex couples, so we might expect to see effects of the laws on opposite-sex couples as well. The results section reports the β_3 and β_6 coefficients from various specifications of Equation (2.2). In only two cases are the β_3 coefficients significant, which is what we would expect from random chance in a series of placebo regressions. This is reassuring that something spurious is not causing the results we see with same-sex couples.

A potential issue with including any variable to distinguish between married and unmarried opposite-sex couples is that the laws may alter which category a couple fits into if the extensions in legal recognition affect marriage decisions for people in that state. In Dillender (2013), I find that extending marriage to same-sex couples in the United States has had no effect on opposite-sex marriage rates, but I do find that extending non-marriage legal recognition to opposite-sex couples results in a decline in the opposite-sex marriage rate, likely because a portion of people who would have gotten married opt for non-marriage legal recognition instead. When considering how legal recognition laws affect the stocks of marriage, I find little or no effect of any type of legal recognition laws, suggesting the pools of married couples are slow to change in any significant ways because there is already a high stock of married people.

Although the estimation strategy allows for states that offer legal recognition to have different starting levels for the dependent variables, it still assumes that states that offer legal recognition are comparable to those that do not. This assumption would be violated if states that offer legal recognition to same-sex couples have a different time trend specific to same-sex couples. Later, I show the point estimates are robust to being more careful in choosing the control group by comparing states with legal recognition to states that have not passed same-sex marriage bans.

2.5 Results

2.5.1 Labor Force Participation

I begin by showing how legal recognition affects labor force participation. Figure 2.1 shows an event study for how the within-couple difference in labor force participation changes for female same-sex couples, male same-sex couples, and unmarried opposite-sex couples before and after same-sex couples can enter into legal recognition. The coefficients are noisy for same-sex couples, but there appears to be no effect of legal recognition for male same-sex couples. For female same-sex couples, however, there appears to be an increase in the number of couples with one member in the labor market and one member not in the labor market. For unmarried opposite-sex couples, the within-couple difference in labor force participation does not appear to change after same-sex couples can enter into legal recognition.

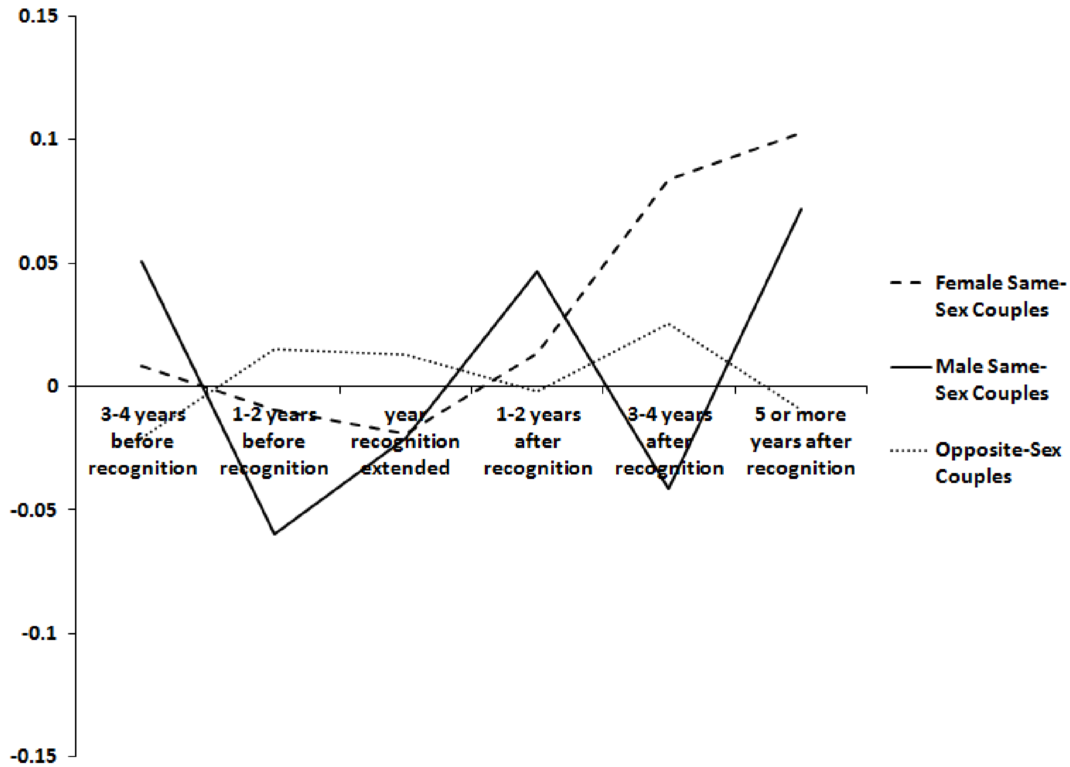

Figure 2.1: This graph shows how the within-couple difference in labor force participation changes after same-sex couples can enter into legal recognition after controlling for *state* and *year* interactions, *state* and *year* interacted with $same-sex$ and *cohab*, and controls for *age*, *race*, *education*, and spouses' demographic characteristics.

Table 2.5 contains the results from estimating various specifications of Equation (2.2). Standard errors, which are corrected for clustering at the state level, are in parentheses. Column 1 of Table 2.5 shows the results for estimating Equation (2.2) using whether or not the woman is in the labor force as the dependent variable. The coefficient of -0.079 indicates labor force participation has fallen for women in same-sex couples by 7.9 percentage points. This represents a 9 percent decrease in labor force participation. Columns 2, 3, and 4 display the coefficients when the dependent

Table 2.5: Labor Force Participation of Female Same-Sex Couples

	Labor force participation	Both members in labor force	One member in labor force	Neither member in labor force	Couples with children younger than 5: Both members in labor force	Couples with children younger than 5: One member in labor force	Couples having children younger than 5
Recognition*same-sex	-0.079**	-0.122**	0.102*	0.021	-0.445**	0.478**	-0.020
	(0.039)	(0.057)	(0.057)	(0.037)	(0.190)	(0.199)	(0.053)
Recognition*cohabit	0.024**	0.013	-0.014	0.002	-0.001	0.005	-0.003
	(0.011)	(0.011)	(0.012)	(0.006)	(0.042)	(0.041)	(0.009)
Unit	individual	couple	couple	couple	couple	couple	couple
n	428,353	427,499	427,499	427,499	69,909	69,909	427,499

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include *state* and *year* interactions, *state* and *year* interacted with $same - sex$ and *cohab*, and controls for *age*, *race*, and *education*. Couple-level regressions also control for spouses' demographic characteristics.

variables are both members in the labor force, only one member in the labor force, and neither member in the labor force, respectively. In column 2, the dependent variable equals 1 if both members of the couple are working and 0 otherwise. The likelihood of both members of a same-sex couple being in the labor force falls by 12.2 percentage points. The likelihood of only one member being in the labor force rises by almost the same amount, and there appears to be no major effect on the number of same-sex couples with neither member in the labor force. This suggests that after legal recognition, women in same-sex couples move from an arrangement where both members work to one where only one member of the couple works. In states that allow same-sex couples to enter into legal recognition, 26 percent of female same-sex couples have one member in the labor force, while 70 percent have both members in the labor force. In these same states, 28 percent of married opposite-sex couples have one member in the labor force, while 68 percent of couples have both members in the labor force. This suggests that while female same-sex couples do not have identical labor force participation arrangements after legal recognition, their labor force arrangements resemble opposite-sex couples much more than they did before they were granted access to legal recognition. A two-sample t-test cannot reject that they are the same at the 10 percent level.

There are a lot of reasons a person would stop working. A few examples are to take care of children, to take care of elderly parents, to enjoy leisure time, and to deal with health issues. Most of these are beyond the scope of this data set, but we can test for larger effects for those with children. In columns 5 and 6, the sample includes only couples with a child younger than 5 present in the household. The dependent

Table 2.6: Labor Force Participation of Male Same-Sex Couples

	Labor force participation	Both members in labor force	One member in labor force	Neither member in labor force
Recognition*same-sex	-0.018	-0.037	0.030	0.007
	(0.032)	(0.066)	(0.065)	(0.014)
Recognition*cohabit	-0.015	0.013	-0.014	0.002
	(0.009)	(0.011)	(0.012)	(0.006)
Unit	individual	couple	couple	couple
n	428,335	427,490	427,490	427,490

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include *state* and *year* interactions, *state* and *year* interacted with $same - sex$ and *cohab*, and controls for *age*, *race*, and *education*. Couple-level regressions also control for spouses' demographic characteristics.

variable in column 5 is a 1 if both members of the couple are in the labor force. The dependent variable in column 6 is a 1 if only one member of the couple is in the labor force. The coefficients in columns 5 and 6 of -0.445 and 0.478, respectively, on $Recognition * same - sex$ are dramatically higher for these couples. In column 7, I show that there is no evidence of any changes in the number of couples with young children as the result of legal recognition, suggesting that changes in family structure are not responsible for the changes in labor force participation.

We would expect small effects on unmarried opposite-sex couples if any at all since only a few states allow unmarried opposite-sex couples to enter into these new unions. Out of the coefficients in Table 2.5, the coefficient on $Recognition * cohabit$ is significantly different from zero only in the specification with labor force participation as the dependent variable. The sign of this coefficient is the opposite sign as the coefficient on $Recognition * same - sex$. As discussed before, out of the many coefficients on $Recognition * cohabit$ reported in this paper, only two of them are statistically different than zero. Although it may be the case that more women in unmarried opposite-sex couples are working compared to women in married opposite-sex couples, we would expect two false rejections of the null hypothesis arising from random chance from running many regressions.

The coefficient on the average effect on labor force participation in column 1 for men in same-sex couples is -.018. When I examine couples changing labor force arrangements, the coefficients on $Recognition * same - sex$ are also insignificant. If there are effects of legal recognition on male same-sex couple's labor force participation, they appear to be small. Since so few male same-sex couples have young children, I cannot restrict the sample as I did with women. The coefficients on $Recognition * cohab$ are all close to zero and insignificant as well.

2.5.2 Health Insurance

Now I show that changes in health insurance appear to suggest that women are able to change their labor force participation because of the ability to receive insurance through a spouse's employer. Figure 2.2 displays an event study for how the likelihood of a couple having at least one member with insurance through a spouse's employer changes after legal recognition is granted for same-sex couples for female same-sex couples, male same-sex couples, and unmarried opposite-sex couples. As before, the coefficients for same-sex couples are noisy. However, there is a noticeable increase after legal recognition for women in same-sex couples but not for men in same-sex couples or unmarried opposite-sex couples.

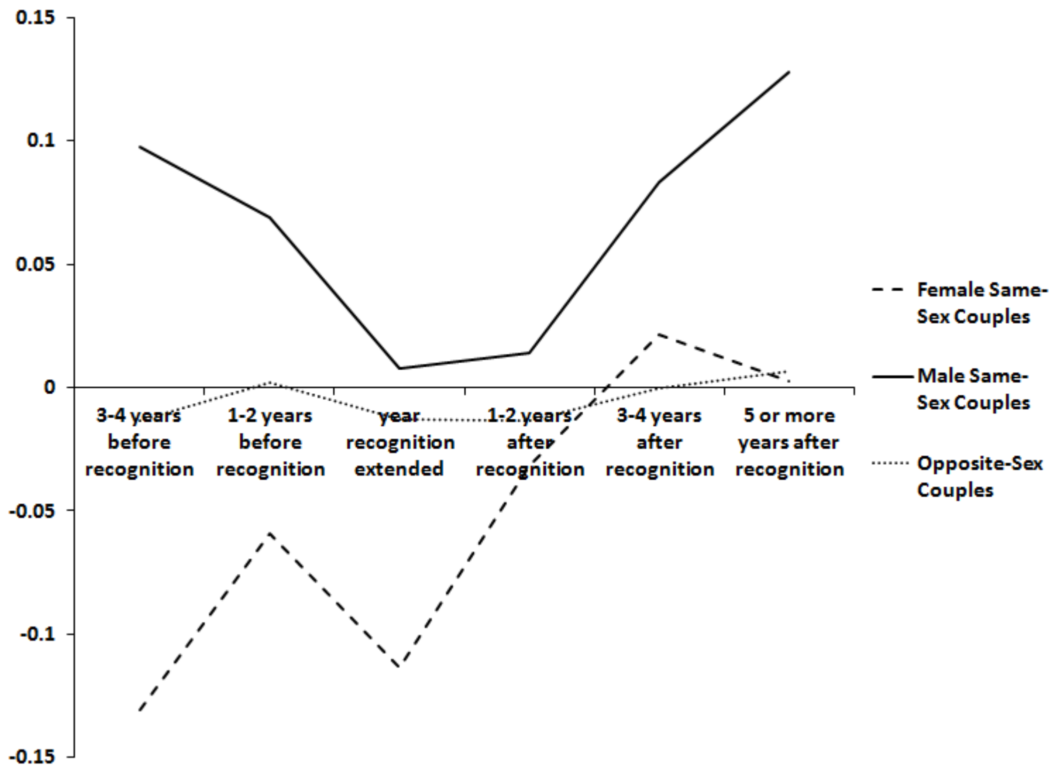

Figure 2.2: This graph shows how the number of couples taking advantage of the ability to receive insurance through a spouse's employer changes after same-sex couples can enter into legal recognition after controlling for *state* and *year* interactions, *state* and *year* interacted with $same-sex$ and *cohab*, and controls for *age*, *race*, *education*, and spouses' demographic characteristics.

The basic results for women are shown in Table 2.7. The first column of Table 2.7 shows the estimates for Equation (2.2) with insurance through a spouse's employer as the dependent variable. The coefficient on $Recognition * same-sex$ is .067, meaning women in same-sex couples experience a 6.7 percentage point increase in employer-sponsored health insurance through a spouse's employer after legal recog-

Table 2.7: Effects of Legal Recognition on Health Insurance for Female Same-Sex Couples

	Insurance through spouse's employer	Insurance through own employer	Privately purchased insurance	Public insurance	Any insurance
Recognition*same-sex	0.067**	-0.078	0.000	0.014	-0.016
	(0.031)	(0.062)	(0.035)	(0.032)	(0.040)
Recognition*cohabit	0.006	0.013	-0.010	-0.015	0.006
	(0.009)	(0.016)	(0.008)	(0.008)	(0.014)
Unit	individual	individual	individual	individual	individual
n	428,353	428,353	428,353	428,353	428,353

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include *state* and *year* interactions, *state* and *year* interacted with $same-sex$ and *cohab*, and controls for *age*, *race*, and *education*.

nition. The average level of health insurance through a spouse's employer for women in same-sex couples is 2 percent in the first four years of my data (before any legal recognition for same-sex couples was passed) and the average for married women in opposite-sex couples is 49 percent. This suggests legal recognition has accounted for about 15 percent of the gap.

In the second column, the dependent variable is insurance through one's own employer. Here we see that women in same-sex couples are less likely to have health insurance through their own employers after legal recognition. The coefficient of -0.078 on $Recognition * same - sex$ is insignificant at the conventional levels, but it has approximately the same absolute value as the coefficient in column 1, suggesting women in same-sex couples are in fact switching their sources of health insurance coverage. Both of these coefficients are largely similar to the estimate of the reduction in labor force participation found earlier.

Table 2.8: Effects of Legal Recognition on Health Insurance for Male Same-Sex Couples

	Insurance through spouse's employer	Insurance through own employer	Privately purchased insurance	Public insurance	Any insurance
Recognition*same-sex	-0.004	0.014	-0.039	-0.031	-0.046
	(0.026)	(0.039)	(0.045)	(0.032)	(0.038)
Recognition*cohabit	0.011	-0.031*	0.007	0.010	0.000
	(0.007)	(0.016)	(0.008)	(0.009)	(0.015)
Unit	individual	individual	individual	individual	individual
n	428,335	428,335	428,335	428,335	428,335

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include *state* and *year* interactions, *state* and *year* interacted with $same - sex$ and *cohab*, and controls for *age*, *race*, and *education*.

The next two columns show results for specifications with privately purchased health insurance and public health insurance as the dependent variables. We would expect these coefficients to be negative if people switch from either of these sources of insurance to coverage through their spouses' employers. All of the coefficients are close to zero and statistically insignificant.

In column 5, I examine the effects of these laws on women in same-sex couples having health insurance from any source. Not surprisingly given the counteracting effects, I find no evidence that women in same-sex couples experience a change in their overall health insurance coverage after the passage of the laws. The coefficient on $Recognition * same - sex$ is insignificant and relatively close to 0. All of the coefficients on $Recognition * cohab$ are insignificant and close to zero for women.

Table 2.8 displays the health insurance results for men. Unlike with women, all of the coefficients on $Recognition * same - sex$ are insignificantly different from zero. Privately purchased insurance may fall a little, which may have led to men in same-sex couples to be less likely to have any source of coverage to fall, but again, these coefficients are both statistically indistinguishable from zero.

The coefficient on $Recognition * cohab$ is statistically different from zero only when insurance through one's own employer is the dependent variable. This may indicate that men in opposite-sex couples are slightly less likely to have insurance through their own employer. However, as discussed before, we would expect a few false rejections of the null when many regressions are run.

2.6 Robustness and Other Issues

I next verify the robustness of the results to various data choices as well as to the control group chosen. I also look at the effects of legal recognition on migration patterns to verify that couples changing state of residence is not driving the results. In the robustness checks that follow, I focus on the specifications with the dependent variables being only one member of the couple in the labor force and having insurance through a spouse's employer; however, the results are similar for all dependent variables.

Age Differences between Couples

The method for identifying same-sex couples before 2007 identified two sets of three people who reported being unmarried partners. When I examined the ages of these people, it appears that they are parents and children who were misclassified. For example, in one case, two people who were about 25 years younger than the head of the household were reported as being his unmarried partners. These suspicious data points were not included in the sample. To limit the possibility that other people are

Table 2.9: Robustness

	One in member in labor force	Insurance through spouse's employer	Move states	One in member in labor force	Insurance through spouse's employer	One in member in labor force	Insurance through spouse's employer
Women							
Recognition*same-sex	0.121*	0.072**	0.027	0.148	0.048	0.104*	0.083**
	(0.063)	(0.035)	(0.026)	(0.113)	(0.035)	(0.054)	(0.035)
Recognition*cohabit	-0.009	0.007	-0.002	-0.005	-0.004	-0.013	0.006
	(0.014)	(0.009)	(0.004)	(0.016)	(0.008)	(0.013)	(0.009)
Unit	couple	individual	individual	couple	individual	couple	individual
n	425,934	426,653	428,353	160,802	161,151	426,803	427,530
Men							
	One in member in labor force	Insurance through spouse's employer	Move states	One in member in labor force	Insurance through spouse's employer	One in member in labor force	Insurance through spouse's employer
Recognition*same-sex	0.048	0.005	0.003	-0.005	0.037	0.032	-0.009
	(0.069)	(0.033)	(0.021)	(0.066)	(0.024)	(0.066)	(0.032)
Recognition*cohabit	-0.009	0.010	0.004	-0.006	0.014	-0.013	0.009
	(0.014)	(0.007)	(0.005)	(0.016)	(0.010)	(0.012)	(0.007)
n	425,924	426,633	428,335	160,830	161,207	426,706	427,426
Unit	couple	individual	individual	couple	individual	couple	individual
Sample changes	age diff > 15 dropped			states with bans dropped		non-HH removed	

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses. All specifications include *state* and *year* interactions, *state* and *year* interacted with $same - sex$ and *cohab*, and controls for *age*, *race*, and *education*. Couple-level regressions also control for spouses' demographic characteristics.

misclassified in this way, I restrict the sample to include only unmarried couples in which the age difference between members of the couple is fifteen years or less. The estimates are reported in columns 1 and 2 of Table 2.9. This type of misclassification should bias the estimates towards zero. The coefficients on $Recognition * same - sex$ increase slightly for both men and women when the within-couple difference in labor force participation and insurance through a spouse's employer are the dependent variables. This suggests misclassification is not a major concern.

Migration

A reasonable hypothesis might be that gay individuals move to places with legal recognition laws. While how legal recognition laws affect migration patterns for gay individuals is an interesting question in its own right, the estimates presented in this paper would be affected if gay people move as a result of legal recognition laws and this movement is related to health insurance needs.

To examine the issue of migration, I take advantage of a variable in the CPS that measures respondents' migration patterns. I create a variable that equals 1 if the respondent has moved between states in the past year and 0 otherwise and estimate Equation (2.2) using this variable as the dependent variable. A positive coefficient on $Recognition * same - sex$ would indicate that people in same-sex couples in states with legal recognition are more likely to have moved than people in married couples. A negative coefficient would indicate that people in same-sex couples are less likely to have moved.

The results are reported in column 3 of Table 2.9. For both men and women, the estimates are insignificant and close to zero. This suggests the majority of the effect cannot be driven by same-sex couples recently migrating to states that would provide them with legal recognition.

Comparing to States without Constitutional Bans

The empirical strategy used all states that did not extend legal recognition to same-sex couples as the control group. Although the estimating equation allowed for states that extended legal recognition to same-sex couples to have different starting points, a potential concern is that states that extend legal recognition change in unobservable ways differently than states that do not extend legal recognition and that these changes drive the results.

In Figures 2.1 and 2.2, I considered what happened in the years before legal recognition and the year of recognition and did not find any evidence that there were any effects before legal recognition. As further evidence that unobservable changes in states that pass legal recognition are not driving the results, I alter the control and treatment states in an attempt to make them more comparable. As of March 2011, legal recognition had never come about because of a ballot initiative; however, oftentimes legal recognition is able to come about only because a ballot initiative has not altered a state's Constitution to ban same-sex unions. Thus, states that have not banned legal recognition may be more comparable than those that have passed Constitutional amendments banning same-sex couples from marrying. In columns 4 and 5 of Table 2.9, I restrict the sample to only states that have not passed Constitutional amendments banning same-sex marriage as of March 2011.[10]

Seeing the point estimate fall to zero when using this new control group would raise a concern that unobservable changes in states that extend legal recognition to same-sex couples drive the results. The results are no longer significant, likely because the number of treated states has been reduced. However, for women, the coefficient on $Recognition * same - sex$ is slightly higher when the within-couple difference in labor force participation is the dependent variable and only slightly smaller when health insurance through a spouse's employer is the dependent variable. For men, the coefficient on $Recognition * same - sex$ is smaller for the within-couple difference in labor force participation is the dependent variable and higher when health insurance through a spouse's employer is the dependent variable. Even when the control and treated states are altered, the coefficients are largely similar.

Using only Couples with One Member as the Head of the Household

In the previous analysis, I used two methods to identify same-sex couples in the sample. As discussed earlier, the first involved using a recently added question to the CPS that asks unmarried adults in the household with unrelated adults, "Do you have a boyfriend, girlfriend, or partner in this household?" If they respond yes, they are asked to identify the cohabiting partner. Thus, we can identify cohabiting partners regardless of whether or not one is the head of the household since 2007. Before 2007,

[10]The new set of control states is Delaware, Illinois, Indiana, Minnesota, New Mexico, New York, North Carolina, Pennsylvania, Rhode Island, West Virginia, and Wyoming. The new set of treatment states is Iowa, Maine, Maryland, Massachusetts, New Hampshire, Vermont, Washington, and the District of Columbia.

I used a method for identifying same-sex couples that has been widely used in the literature. Two people were identified as being a same-sex couple if one was coded as being the head of the household, the other was coded as being the unmarried partner of the head of the household, and they were the same sex. A potential concern is that couples with one member acting as the head of the household are different than couples where neither member is the head of the household.

In columns 6 and 7 of Table 2.9, I replicate the results dropping couples where neither member is the head of the household. The coefficient on $Recognition * same - sex$ is 0.104 for women, which is almost identical to the coefficient in column 2 of Table 2.5. The coefficient of 0.083 on $Recognition * same - sex$ when insurance through a spouse's employer is the dependent variable is only slightly larger than the coefficient from column 1 of Table 2.7. The coefficients for men are similar as well.

2.7 Conclusion

The discussion about same-sex marriage and other forms of legal recognition for same-sex couples in the United States has touched on both civil and economics rights and advantages. However, little has been known about what the economic impacts of legal recognition have actually been for same-sex couples. In this paper, I examined how legal recognition has affected labor force participation and health insurance for same-sex couples.

Female same-sex couples are more likely to have only one member in the labor force after they can enter into legal recognition. This results in a decline in overall labor force participation for women in same-sex couples. The ability to receive health insurance through a spouse's employer appears to facilitate this change as women in same-sex couples are more likely to have insurance through a spouse's employer and less likely to have it through their own employer after legal recognition. These results are robust to a variety of specifications and data choices.

Men in same-sex couples experience no major change after legal recognition in either their insurance coverage or their labor force participation. Part of this may be due to the fact that male same-sex couples male same-sex couples are less likely to have children than women in same-sex couples.

In the absence of legal recognition, female same-sex couples' labor force participation and health insurance coverage resemble unmarried opposite-sex couples more than married opposite-sex couples. After being granted access to legal recognition, however, female same-sex couples start to look more like married opposite-sex couples, suggesting marriage may be responsible for some of the differences between married and unmarried opposite-sex couples.

Chapter 3

The Death of Marriage? The Effects of New Forms of Legal Recognition on Marriage Rates

Does the value of an institution depend on who else participates in that institution? Many people argue that this is the case with marriage and that allowing same-sex couples to marry reduces the value of marriage to opposite-sex couples.[1] Marriage is of interest because it serves as both a social and legal contract that facilitates family decision-making and provides legal and cultural safeguards.[2] In economic models of marriage, people choose to marry when the benefits of being married outweigh the costs. As a result, if marriage becomes less valuable, marriage rates will decline. In this paper, I analyze the effects of changing legal recognition laws on marriage rates in the United States.

The potential effect on opposite-sex marriage of allowing same-sex couples to marry is theoretically ambiguous. Allowing same-sex couples to marry could lower opposite-sex marriage rates if it severs the link between marriage and childbearing or

[1] For example, in June 2011, then presidential candidate Rick Santorum stated that allowing same-sex couples to marry would "cheapen marriage and make it into something less valuable" (The Des Moines Register (2011)). In 2004, James Dobson stated "[Gay people] want to destroy the institution of marriage. [Same-sex marriage] will destroy marriage" (Snyder (2004)). The end-of-marriage argument was largely the rationale behind Proposition 8, the California Constitutional amendment that restricted marriage to being a union between a man and a woman.

[2] Much of the work on marriage and economics stems from Becker (1973) and Becker (1974).

if it reduces any value of marriage that comes from its exclusivity (Kurtz (2004)). Alternatively, allowing same-sex couples to marry could increase opposite-sex marriage rates by increasing interest in the institution of marriage or by reducing the pressure on employers to provide marriage-like benefits to cohabiting couples (Rauch (2004) and Trandafir (2012)). Additionally, opposite-sex couples may only want to become married when marriage or some form of legal recognition is available to all couples if they feel excluding couples makes marriage a discriminatory institution.

Few papers have studied how allowing same-sex couples to marry affects marriage rates. Langbein and Yost (2009) use data on the stock of marriages and find the number of married people in a state does not change when same-sex couples are allowed to marry. However, the stock of marriages may be slow to change even if marriage rates change immediately. Another issue is that Langbein and Yost use data through 2004, which is when states began allowing same-sex couples to marry. This means the effect on marriage is identified by very few state-year observations.

Trandafir (2012) studies the effects of a Netherlands law that allows same-sex couples to marry and a separate law that allows all couples to enter into registered partnerships, which provide similar benefits to marriage. Trandafir finds suggestive evidence that marriage rates rise after all couples can enter into registered partnerships but fall after same-sex couples can marry. Although women are less likely to be married after same-sex couples can marry, Trandafir concludes the experience of the Netherlands suggests no major effects of changing legal recognition laws on overall marriage rates since controlling for heterogeneity greatly reduces the coefficient. Since both of these laws changed for the country as a whole with only a few years in between, Trandafir has difficulties disentangling the effects of the two laws. Furthermore, people in the United States, who are culturally very different than residents of the Netherlands, may react in a different manner.

The advantage of studying legal recognition changes in the United States is that it provides a variety of policy experiments happening at different points in time over the last decade. In some states, same-sex couples are allowed to marry, while in other states they are allowed to enter into newly created forms of legal unions instead of marriage. Only same-sex couples can enter into the new forms of unions in some states, while in others all couples can enter into the non-marriage legal recognition.

Opening new forms of legal recognition to opposite-sex couples could result in lower marriage rates if some couples prefer an alternate form of legal recognition if they are not religious because they feel marriage has religious meaning. An issue with this is that domestic partnerships do not offer the federal benefits of marriage because of the Defense of Marriage Act, so people entering into domestic partnerships instead of marriage would have fewer legal benefits than they would if they were married. This would suggest increasing the benefits of domestic partnerships may make couples better off, although it might also cause more couples to choose to enter into domestic partnerships instead of marriage.

To analyze the effects of the changes in legal recognition, I use two data sources. The first is a state-level panel data set that I construct containing marriage rates, legal changes, and other state characteristics. The advantages of this data set are that the marriage rates come directly from the states and account for every marriage occurring in the state in a given year. As a result, I am able to consider how these laws affect both overall marriage rates and opposite-sex marriage rates. However, there are a few disadvantages of using this type of data. The first is that people often marry in states other than where they reside, which would confound any estimation strategy using marriage rates in a state. Second, we cannot account for individual heterogeneity. Finally, the legal changes could affect the stock of marriages without affecting the flow if couples exit marriage after the legal changes. To deal with these issues, I use individual-level data from the Current Population Survey and examine how the stock of married couples changes in response to legal recognition laws.

With both data sets, I estimate difference-in-differences models as well as models with flexible time effects, which allow the effects of legal recognition changes to vary over time. Allowing the effects of these laws to vary over time is important for several reasons. First, marriage decisions are typically made years in advance, meaning we might not see the effects of these laws immediately. Second, we can test for effects before changes in legal recognition. This allows us to examine if differing time trends before the legal changes are a concern and to see if there is any evidence that people respond after the laws are passed but before they are enacted. Finally, the number of same-sex couples marrying is likely to be at its highest in the first few years because of pent-up demand. Time-flexible specifications can help us compare immediate effects to longer run effects.

I find that allowing same-sex couples to get married increases the overall marriage rate, but this increase appears to be driven entirely by same-sex couples marrying. Regardless of the identification strategy used, there is no evidence that allowing same-sex couples to marry has altered marriage for opposite-sex couples. Opposite-sex couples do, however, take advantage of the new forms of legal recognition when they are available to them. Marriage rates fall by about 10% whenever non-marriage legal recognition is available to opposite-sex couples. These results are robust to a number of specifications, and I find no evidence that national marriage rates were affected after the first state began allowing same-sex couples to marry in 2004.

The outline of the paper is as follows. The next section discusses the changes in legal recognition that have taken place in the United States. Section 2 discusses the construction of the data sets and the identification strategies. Section 3 presents the main results of the paper. Section 4 considers the robustness of the results, and Section 5 concludes.

3.1 Changes in Legal Recognition

Trends in marriage rates show that marriage rates have been falling nationally for may years. Figure 3.1 shows national marriage rates for the time period studied. The downward trend in marriage rates during this time period started in the early 1980s, meaning we cannot simply compare what happens in a state after legal recognition and necessitates that we account for a national time trend by having a control group that would be subject to the same time trend. Table 3.1 shows state changes in legal recognition for couples. Several states have changed their marriage laws to allow same-sex couples to marry. Others have created alternate forms of legal recognition called civil unions or domestic partnerships.[3]

[3]Colorado allows people to designate beneficiaries. Since these types of unions do not imply a romantic relationship–any two unmarried people can enter into designated beneficiary agreements

Figure 3.1: National Marriage Rates

These new forms of legal recognition are designed to provide the same state-level benefits as marriage. The rights granted to couples through these different types of unions vary by state. Common rights covered include hospital visitation rights, family leave for a sick or dying partner, the right for partners to share a nursing home room, the ability to inherit a partner's estate in the absence of a will, and immunity from testifying against a partner in court. Due to the 1996 Defense of Marriage Act, even when same-sex couples are allowed to marry, they do not receive any of the federal benefits of marriage. Likewise, civil unions and domestic partnerships are not

including friends and siblings–and do not offer most of the benefits of marriage, I do not code Colorado as providing alternate recognition. All results are robust to dropping Colorado or estimating a separate coefficient for the effect of designated beneficiary agreements.

Table 3.1: Extensions of Legal Recognition by State

	Same-Sex Marriage	Only Same-Sex Couples	Opposite-Sex Couples if ≥ 62	All Couples
California			9/22/1999 L	
District of Columbia				1/1/2002 L
Colorado				7/1/2009 L
Connecticut	11/12/2008 J	10/1/2005 L		
Iowa	4/2/2009 J			
Maine				7/30/2004 L
Maryland				7/1/2008 L
Massachusetts	5/17/2004 J			
Nevada				10/1/2009 L
New Hampshire		1/1/2008 L		
New Jersey			7/10/2004 L	
Oregon		2/4/2008 L		
Vermont	9/1/2009 L	7/1/2000 J		
Washington			7/22/2007 L	
Wisconsin		8/3/2009 L		

J indicates that the law came about through the judicial system, while L indicates that it was passed by a state legislature.

recognized by the federal government even when they are available to opposite-sex couples. The federal benefits of marriage include social security benefits for surviving spouses, the ability to file income taxes jointly, which may reduce the overall tax rate the couple faces, no estate taxes on inheriting a deceased spouse's assets, and the ability to petition for a spouse to immigrate to the United States.[4]

While the fact that the unions are not recognized by the federal government may hurt same-sex couples, it is a possible advantage for many opposite-sex couples. Widows and widowers are eligible to receive the social security benefits their spouses would have received if they don't remarry by the age of 60. Thus, civil unions and domestic partnerships can provide opposite-sex couples with state-level protection while not jeopardizing their social security survivor benefits. This is likely the rationale for several states offering these state forms of legal recognition to opposite-sex couples as long as at least one member of the couple is at least 62.

For the purposes of this paper, I classify the laws into three different categories. The first is those states that allow same-sex couples the right to be married. The second category is states that allow all same-sex couples and all opposite-sex couples the right to enter into the new form of legal recognition. For the third category, I combine laws that provide new forms of legal recognition to same-sex couples only and the laws that allow opposite-sex couples to enter into the new forms of unions if at least one member of the couple is at least 62.[5] I refer to these states as states that

[4]For a complete listing of federal benefits of marriage, see the Government Accountability Office's website, http://www.gao.gov/new.items/d04353r.pdf.

[5]Results are robust to the inclusion of the laws separately. I combine the laws because the coefficients on the two types of laws are similar if I estimate the effects separately.

provide new forms of legal recognition to same-sex couples only. For ease of discourse, I will refer to all of the new forms of unions as domestic partnerships.

3.2 Data Sources and Identification Strategy

3.2.1 Data

To examine the impact of legal recognition changes on marriage, one can look at either stock or flow measures. The stock measure is the total number of marriages, and the flow measure is the number of people entering into marriage. Any change in the value of marriage should affect both, but stock measures might be affected at a slower rate. Because of the structure of my data, I construct a state-level panel to examine the flows of marriage and use the March CPS to examine the stocks of marriages.

The data containing the marriage rate per 1,000 individuals for each state in a given year come from the Centers for Disease Control and Prevention (CDC) for 1995 to 2010. As is common in the literature, I use the log of state-level marriage rates, which will allow us to interpret the coefficients as percent changes in marriage rates.[6] All states have reported marriage rates for all years except for Oklahoma, which did not report marriage rates for a few of the years studied.

Marriage rates from the CDC are formed using all marriages in a given state and year. For states that allow same-sex couples to marry, I obtain the number of same-sex marriages happening in a year from the state health departments, which

[6] For examples, see Bitler et al. (2004) and Brien et al. (2004). The results are not sensitive to this specification choice; results are similar if I use the marriage rates.

keep data on same-sex marriages but do not report this data to the CDC. I then subtract this number from the total number of marriages occurring to calculate the opposite-sex marriage rate.[7]

Table 3.2: Descriptive Statistics

	Mean	St. Dev.
Marriage Rates	8.98	8.16
Unemployment Rates	0.06	0.02
% Female	0.52	0.01
% of High School Graduates	0.33	0.04
% of People with Some College	0.26	0.04
% of College Graduates	0.17	0.03
% Black	0.11	0.12
% White	0.82	0.15
% Age 21 to 40	0.27	0.02
% Age 41 to 60	0.26	0.02
% Age 61 and above	0.14	0.03

There are 811 observations.

I supplement the data on marriage rates with various state-level controls calculated using the March CPS. For each state during each year of the data, I calculate the percentage of people 25 and older with high school degrees, the percentage who have completed some college, and the percentage who have completed college. I also calculate the percentage of people in the labor force who are unemployed. I control for the percentage of people in three broad age groups, ages 21 to 40, ages 41 to 60, and people older than 60. Finally, I calculate the percentages of people who are white and black and the percentage of the people who are female. I control for these

[7]Washington, D.C. does not keep statistics on the number of same-sex marriages; as a result, Washington, D.C. is dropped in the analysis of opposite-sex marriages.

demographic characteristics in certain specifications to make sure that changes in demographic characteristics are not driving any of the results. The descriptive statistics are shown in Table 3.2.

In addition to using the March CPS to account for demographic changes in the construction of the state-level panel data set, I also use the March CPS from 1995 to 2011 to examine the stock of marriages. With this dataset, I control for race, gender, and a cubic in age. I cannot identify the same-sex couples who enter into marriage in the CPS because the CPS codes all same-sex couples as being unmarried partners, so I focus only on the stock of opposite-sex marriages.[8]

3.2.2 Identification Strategy

I estimate both simple difference-in-differences models as well as models that allow the effects of legal recognition changes to vary over time. An issue with the time-flexible models is that many of these laws have been passed only recently and many states have not expanded their definitions of legal recognition. This results in large standard errors, as some of the coefficients are identified using only a few observations. By examining the more aggregated difference-in-differences estimator, we can better identify the average effects over time even though we no longer have estimates at each point in time.

I estimate two main equations. The first provides us with the difference-in-

[8]I drop same-sex couples from the sample as well as any couples who have had their marital status or gender changed. Before 2010, the CPS changed the sex of the spouse if two people of the same sex report being married. Beginning in 2010, the CPS changed the marital status. The results are very similar if I do not try to account for same-sex couples.

differences estimator:

$$y_{st} = \phi_t + v_s + X_{st}\alpha + \sum_{j \in J} \beta_j L^j_{st} + \epsilon_{st}, \tag{3.1}$$

where y is the log of the marriage rate per 1,000 people, s indexes the state, t indexes the year, ϕ is a vector of time effects, v is a vector of state effects, X is a vector with the average demographic characteristics for each state in a given year, L^j_{st} is an indicator variable equal to 1 in a state after a law of type j was passed, and ϵ is the state-level error term. Again, there are three potential types of laws: 1) those allowing same-sex couples to marry, 2) those allowing all couples to enter into new forms of recognition, and 3) those allowing same-sex couples and only same-sex couples to enter into new forms of recognition. The β coefficients provide us with the effect of legal recognition changes averaged over time.

We also want to be able to distinguish immediate effects of the laws from later effects. To do this, I estimate a model of the following form:

$$y_{st} = \phi_t + v_s + X_{st}\alpha + \sum_{k \in K} \sum_{j \in J} \beta_{jk} L^{jk}_{st} + \epsilon_{st}, \tag{3.2}$$

where L^{jk}_{st} is an indicator variable equal to 1 in the kth period after a law of type j was passed and the other variables are defined as before. Because many of these laws are recent, we will have a difficult time identifying individual year effects for high $k's$. Therefore, I look at $k = \{-1, 0, 1, 2\}$ and $k \geq 3$. The laws were passed in the year $k = 0$. We can interpret β_{jk} as being the effect of a law change of type j k years after its passage.[9]

[9] A similar econometric model was used by Wolfers (2006) to study the effects of divorce laws.

3.3 Results

3.3.1 Marriage Flows

The results from estimating Equation (3.1) with the state level data are shown in Table 3.3. In the first two specifications, the dependent variable is the log of the overall marriage rate. In the next two specifications, the dependent variable is the log of the opposite-sex marriage rate. The first and third specifications control for demographic characteristics of the states, while the second and fourth do not.

From column 1, we can see that allowing same-sex couples to marry increased the overall marriage rate by about 13.7%. Controlling for demographic characteristics causes the coefficient to decrease by less than 1 percentage point to 12.8%. However, we must be careful in interpreting these results. States do not have residency requirements for marriage, and reports of same-sex couples in states where same-sex couples cannot marry travelling to states where they can marry are common.[10] We would not expect marriage rates to increase by this much nationally if same-sex couples were allowed to marry across all states. Similarly, we would not expect the increases to be this high as more states allow same-sex couples to marry.

The coefficient on marriage for same-sex couples in column 3 where the dependent variable is the opposite-sex marriage rate is insignificant and close to zero. Controlling for demographics in column 4 changes the coefficient very little. This indicates that allowing same-sex couples to marry has no effect on opposite-sex marriage rates. Since the marriage rates are defined per 1,000 people, the estimates on

[10]These marriages would typically not be legally recognized in non-same-sex-marriage states because of the Defense of Marriage Act.

opposite-sex marriage would all be biased downward if allowing same-sex couples to enter into legal recognition resulted in same-sex couples moving into a state. Dillender (2013) considers migration of same-sex couples and finds no effects of the laws on the numbers of same-sex couples in a state.

The results, however, do suggest that allowing opposite-sex couples to enter into new forms of partnerships decreases the marriage rate between 9 and 11 percent. The coefficients on domestic partnerships for all couples are significant at the ten percent level in three of the four specifications. This suggests that some opposite-sex couples enter into new forms of unions when they are available instead of entering into marriage. This is important for two reasons. The first is that domestic partnerships are legally inferior to marriage because domestic partnerships do not include any federal benefits. The second is that these results suggest that opposite-sex couples may enter into marriage in the absence of alternate recognition when they would really prefer a non-marriage form of legal recognition.

The coefficients on domestic partnerships for same-sex couples only are slightly positive and significant in two of the specifications. The coefficients are similar in size to the coefficients on marriage in the specifications with the opposite-sex marriage rate as the dependent variable. This suggests that allowing same-sex couples and only same-sex couples to enter into new forms of unions has a marginally significant positive impact on opposite-sex marriage rates.

Table 3.4 shows the results when I estimate Equation (3.2), which allows for time-varying effects of the law changes. Note that the coefficients are not cumulative and that the size and significance of all of the effects are relative to all of the years

Table 3.3: Effects on Marriage Rates

	Overall Marriage Rates		Opposite-Sex Marriage Rates	
Marriage for same-sex couples	0.137***	0.128***	0.018	0.010
	(0.038)	(0.038)	(0.020)	(0.020)
DP for all couples	-0.106*	-0.108*	-0.089	-0.090*
	(0.058)	(0.054)	(0.055)	(0.050)
DP for same-sex couples	0.028*	0.023	0.029*	0.023
	(0.015)	(0.017)	(0.015)	(0.017)
Demographic Controls	no	yes	no	yes
n	811	811	810	810

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses.

more than one year before the passage of the laws. Allowing for time-varying effects reveals that the number of same-sex couples marrying is at its highest in the first year after marriage and then decreases in the following years. Three years after allowing same-sex couples to marry the coefficient on marriage for same-sex couples is approximately 9.5% when controls are not included and 8% whenever they are. When the dependent variable is the opposite-sex marriage rate in columns 3 and 4, the coefficients are all slightly positive but close to zero. Of the marriage coefficients, only the coefficient on two years after marriage for same-sex couples is significant. The coefficient is .03, suggesting an increase of only 3% from a base of 7 marriages per 1,000 individuals.

None of the coefficients on domestic partnerships for all couples are significant for the first three years, likely because we have so few observations identifying those effects, but the coefficients on the year of passage of domestic partnerships for all couples and one year after domestic partnerships for all couples are similar in magnitude to the average effect presented in the earlier specification. A slight drop in the estimated coefficients is present on the coefficient on two years after domestic

Table 3.4: Time-Varying Effects on Marriage Rates

	Overall Marriage Rates		Opposite-Sex Marriage Rates	
One year before marriage for same-sex couples	0.024	0.019	0.022	0.020
	(0.034)	(0.030)	(0.032)	(0.031)
Year of marriage for same-sex couples	0.170**	0.161**	0.013	0.007
	(0.065)	(0.062)	(0.031)	(0.031)
One year after marriage for same-sex couples	0.141***	0.131***	0.025	0.016
	(0.028)	(0.027)	(0.022)	(0.02)
Two years after marriage for same-sex couples	0.097***	0.082**	0.030**	0.016
	(0.030)	(0.032)	(0.017)	(0.025)
More than two years after marriage for same-sex couples	0.095***	0.08***	0.029	0.016
	(0.022)	(0.030)	(0.020)	(0.030)
One year before DP for all couples	-0.038	-0.026	-0.028	-0.019
	(0.065)	(0.066)	(0.070)	(0.070)
Year of DP for all couples	-0.103	-0.099	-0.093	-0.091
	(0.075)	(0.073)	(0.076)	(0.074)
One year after DP for all couples	-0.114	-0.098	-0.102	-0.088
	(0.091)	(0.080)	(0.092)	(0.080)
Two years after DP for all couples	-0.049	-0.058	-0.033	-0.046
	(0.032)	(0.036)	(0.033)	(0.039)
More than two years after DP for all couples	-0.135*	-0.146*	-0.110	-0.115*
	(0.076)	(0.081)	(0.068)	(0.066)
One year before DP for same-sex couples only	0.011	0.003	0.010	0.002
	(0.017)	(0.017)	(0.016)	(0.017)
Year of DP for same-sex couples only	0.023	0.010	0.021	0.010
	(0.017)	(0.018)	(0.016)	(0.017)
One year after DP for same-sex couples only	0.005	0.008	0.005	0.007
	(0.025)	(0.026)	(0.023)	(0.026)
Two years after DP for same-sex couples only	0.032**	0.025	0.033**	0.027*
	(0.014)	(0.015)	(0.014)	(0.016)
More than two years after DP for same-sex couples only	0.048*	0.038	0.050***	0.041
	(0.026)	(0.034)	(0.026)	(0.034)
Demographic Controls	no	yes	no	yes
n	811	811	810	810

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses.

partnerships for all couples, but the coefficients on more than two years after domestic partnerships for all couples are all between -14.6% and -11%. The coefficients on more than two years after domestic partnerships for all couples are significant in three out of four of the specifications. There is no evidence of effects before any of the laws were enacted. Although we cannot reject that all of the coefficients on domestic partnerships for all couples are the same, these results do illustrate how using only a few years of data may not be enough to estimate the effects of allowing opposite-sex couples to enter into domestic partnerships.

The coefficients on year of domestic partnerships for same-sex couples only and year after domestic partnerships for same-sex couples only are all close to zero. The coefficients on two years after domestic partnerships for same-sex couples only and more than two years after domestic partnerships for same-sex couples only are positive, and some of the coefficients are significant. Although it is difficult to draw strong conclusions from these coefficients, there appears to be no evidence that allowing same-sex couples to enter into legal unions has a negative effect on opposite-sex marriage.

3.3.2 Marriage Stocks

I next use data from the March CPS to examine the stock of marriages. There are a number of advantages of using the CPS data. The first is that I am able to account for individual heterogeneity. Second, I can address another potential concern of the earlier analysis that stems from the fact that many people do not get married in the states in which they reside. This may be because certain states are marriage destinations or because people want to marry in the state where their family lives. If seeing same-sex couples marrying really does lessen the value of marriage, we would technically expect the number of people living in the state who choose to get married to go down and not necessarily a change in the number of marriages that take place in the state. A limitation of this data, however, is that I am only able to look at opposite-sex marriages due to the coding procedure of the CPS.

Table 3.5: Effects on Marriage Stocks

	Probability of Being Married	
Marriage for same-sex couples	0.012*	0.011*
	(0.007)	(0.006)
DP for all couples	-0.003	0.001
	(0.005)	(0.003)
DP for same-sex couples	0.002	-0.001
	(0.003)	(0.003)
Demographic controls	No	Yes
n	2,249,847	2,249,847

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses.

Table 3.5 shows the difference-in-differences results. The coefficient on marriage for same-sex couples is significantly different from zero at the 10% level, suggesting more people are in opposite-sex marriages after same-sex couples are allowed to marry.[11] The coefficients on domestic partnerships for all couples and domestic partnerships for same-sex couples only are all close to zero and insignificant.

Table 3.6 reports the estimates for the time-flexible models. The coefficients on marriage in the first two years are positive but insignificant and close to zero.

[11] As will be seen in the next section, these estimates are not robust to including state-specific time trends.

Table 3.6: Time-Varying Effects on Marriage Stocks

	Probability of Being Married	
One year before marriage for same-sex couples	-0.006	-0.006
	(0.005)	(0.004)
Year of marriage for same-sex couples	0.002	-0.002
	(0.009)	(0.007)
One year after marriage for same-sex couples	0.008	0.005
	(0.009)	(0.006)
Two years after marriage for same-sex couples	0.012*	0.015***
	(0.007)	(0.004)
More than two years after marriage for same-sex couples	0.026***	0.027***
	(0.003)	(0.003)
One year before DP for all couples	-0.001	0.004
	(0.005)	(0.003)
Year of DP for all couples	-0.013	-0.004
	(0.006)	(0.005)
One year after DP for all couples	-0.005	0.002
	(0.003)	(0.002)
Two years after DP for all couples	-0.003	0.002
	(0.007)	(0.004)
More than two years after DP for all couples	0.003	0.003
	(0.005)	(0.002)
One year before DP for same-sex couples only	-0.003	-0.005
	(0.003)	(0.003)
Year of DP for same-sex couples only	-0.001	-0.001
	(0.006)	(0.004)
One year after DP for same-sex couples only	-0.005	-0.008
	(0.006)	(0.006)
Two years after DP for same-sex couples only	-0.001	-0.005
	(0.008)	(0.007)
More than two years after DP for same-sex couples only	0.006**	0.002
	(0.003)	(0.002)
Demographic controls	No	Yes
n	2,249,847	2,249,847

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses.

Beginning two years after marriage for same-sex couples, the coefficients become larger and significant. The coefficients on more than two years after marriage for same-sex couples are all positive and significant. They suggest the stock of married opposite-sex couples has risen by about 2.6 percentage points after same-sex couples are allowed to marry. The stock of married opposite-sex couples is higher after same-sex couples are allowed to marry.

Although a few of the coefficients on domestic partnerships for all couples and domestic partnerships for same-sex couples only are significant, the general consensus of the coefficients is that there is no effect of either law on the stock of married opposite-sex couples.

The insignificant coefficients on domestic partnerships for all couples may seem at odds with the estimates from the previous section that suggest that allowing opposite-sex couples to enter into alternate forms of recognition lowers the opposite-sex marriage rate. Two factors would minimize the estimated effects of domestic partnerships for all couples on the stocks of opposite-sex married couples from the CPS. The first is that the stock of married people is already high, so even if changes in flow measures take place immediately, the stock measures would be slow to change. The second is that it is not clear how people who enter into domestic partnerships would report their relationship status in the CPS since the only two relationship statuses are unmarried partner and spouse. People reporting that they are spouses if they are domestic partners would mean we would find no effect of extending new forms of legal recognition to opposite-sex couples.

3.4 Robustness

3.4.1 Testing for National Effects

The previously described identification strategy makes the key assumption that legal changes will only impact behavior in states where the laws have been passed. This may be more reasonable with domestic partnerships than same-sex marriage. With domestic partnerships, opposite-sex couples may choose not to enter into marriage and instead take up this new type of legal union only when it is available to them, suggesting state variation should be sufficient. With same-sex marriage, this may not be the case. It could be that same-sex marriage anywhere affects the value of marriage and thus marriage rates everywhere. We cannot identify these types of effects using state variation.

Figure 3.2: Trends in State Overall Marriage Rates

Figure 3.3: Trends in State Opposite-Sex Marriage Rates

To consider the idea that same-sex marriage in any state may have national ramifications, I look at state trends in marriage rates over the last fifteen years. If national marriage rates suddenly drop after same-sex couples begin marrying, we would be concerned that allowing same-sex couples has national ramifications, thus causing the identification strategy used earlier to be wrong. The solid line in Figure 3.2 shows the year coefficients in Equation (3.1) estimated without controlling for the passage of the laws but with the controls previously described. The dashed line shows how these coefficients differ from the year before. The solid line mirrors the shape of the national rates shown earlier. The dashed line hovers around slightly below zero for most of the time period. Figure 3.4.1 shows the equivalent only using opposite-sex marriage rates. In both figures, there seems to be no change in the trend when Massachusetts began allowing same-sex couples to marry in 2004. Marriage rates continue to fall after Massachusetts began allowing same-sex couples but at a similar rate as before. In the past few years, opposite-sex marriage rates have actually risen nationally. Although examining trends can provide no definitive evidence that allowing same-sex couples to marry has no national ramifications, these results do suggest that allowing same-sex couples to marry has not drastically altered marriage rates at a national level.[12]

3.4.2 Unobserved Changes over Time

A second key assumption is that states that alter legal recognition would be changing in similar ways as states that do not alter legal recognition in the absence of legal recognition changes. The identification strategy controls for state heterogeneity that is fixed over time, but a potential concern is that states that offer legal recognition may be changing in unobserved ways differently from states that do not offer legal recognition and that these unobserved changes confound the estimation strategy. When I estimated the time-flexible specifications, I found no effect on marriage rates the year before the passage of the laws. In this section, I verify the robustness of the results to two additional ways to account for unobserved heterogeneity that changes

[12]Trandafir (2012) finds liberal and conservative regions of the Netherlands responded to legal recognition changes in different ways. In other results, I test for different reactions to the Massachusetts ruling for more liberal and more conservative states as measured by the percent of the state population that voted for George Bush in 2004, which is the year Massachusetts began allowing same-sex couples to marry and when one of the main issues in the presidential election was a Constitutional ban on allowing same-sex couples to marry. Bush supported the ban, while his opponent, John Kerry, did not. I find no evidence of differences.

over time. The first involves being more careful in choosing the control group. The second allows states that alter legal recognition to have different time trends than other states.

Choice of Control Group

Changes in legal recognition have never come about through popular votes; instead, legislative action or rulings by state Supreme Courts have led to changes. However, legal recognition can only be extended to same-sex couples in states without Constitutional bans on legal recognition, meaning states without bans on legal recognition might be a better control group than all states without legal recognition.

Table 3.7 replicates the results using states that have neither legal recognition for same-sex couples nor Constitutional bans on same-sex marriage as the control group.[13] I focus on the difference-in-differences results for opposite-sex marriage rates and marriage stocks; however, the results for the time-flexible specifications are similar.

In columns 1 and 2 of Table 3.7, I present results when the dependent variable is the opposite-sex marriage rate. We would be concerned that unobserved state trends were confounding the estimation strategy if the results changed after choosing a more narrowly defined control group. All of the coefficients are similar to those presented before. The main difference is that the coefficients on domestic partnerships for all couples go up in significance levels; the coefficients on marriage are still positive but

[13] The new set of control states is Delaware, Illinois, Indiana, Minnesota, New Mexico, New York, North Carolina, Pennsylvania, Rhode Island, West Virginia, and Wyoming.

Table 3.7: Robustness - Control Group Choice

	Opposite-Sex Marriage Rates		Opposite-Sex Marriage Stocks	
Marriage	0.004	0.027	0.004	0.001
	(0.025)	(0.022)	(0.009)	(0.007)
DP for all couples	-0.104*	-0.090**	-0.008	-0.004
	(0.053)	(0.039)	(0.006)	(0.003)
DP for same-sex couples	0.014	0.039**	-0.002	-0.006
	(0.019)	(0.018)	(0.004)	(0.004)
Demographic controls	No	Yes	No	Yes
State-specific time trends	No	No	No	No
Narrower control group	Yes	Yes	Yes	Yes
n	384	384	1,196,150	1,196,150

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses.

insignificant. This suggests choosing the control group more carefully does not alter the marriage results and actually strengthens the result that allowing opposite sex couples to enter into alternate recognition lowers marriage rates.

In columns 3 and 4, I examine the stocks of marriages. The coefficients on marriage for same-sex couples were significant at the 10% level before. With the new control group, they no longer are, partly because the standard errors go up slightly; however, the point estimates are still positive. This suggests even with a different control group, there is still no evidence allowing same-sex couples to marry reduces the stock of opposite-sex marriages.

State-Specific Time Trends

I next supplement Equation (3.1) with linear state-specific time trends for those states that extend legal recognition. This means identification comes from how marriage rates and stocks change apart from the state-specific trends as well as national trends after legal recognition is extended.

The results are shown in Table 3.8. The first two columns display the results with opposite-sex marriage rates as the dependent variable. The coefficients on both marriage and domestic partnerships for all couples are both higher when state-specific time trends are included. This suggests states that extended legal recognition did tend to have a slightly more negative trend in marriage rates than the nation as a whole. As before, the coefficients on marriage are not statistically different from zero, while the coefficients on domestic partnerships for all couples are negative and statistically different from zero. The coefficients on domestic partnerships for same-sex couples only are insignificant and close to zero.

Table 3.8: Robustness - State-Specific Time Trends

	Opposite-Sex Marriage Rates		Opposite-Sex Marriage Stocks	
Marriage	0.031	0.027	-0.008	-0.006
	(0.021)	(0.025)	(0.008)	(0.006)
DP for all couples	-0.052**	-0.045*	-0.009	-0.006**
	(0.023)	(0.023)	(0.006)	(0.003)
DP for same-sex couples	0.013	0.005	-0.005	-0.006
	(0.015)	(0.015)	(0.006)	(0.005)
Demographic controls	No	Yes	No	Yes
State-specific time trends	Yes	Yes	Yes	Yes
Narrower control group	No	No	No	No
n	810	810	2,249,847	2,249,847

Notes: *, **, and *** indicate significance at 10%, 5%, and 1% respectively. Standard errors are clustered by state and are shown in parentheses.

The next two columns of Table 3.8 show the results with the dependent variable being a one if the individual is married. When the state-specific time trends are included the coefficient on domestic partnerships for all couples becomes negative and significant in one of the specifications but not in the other. The coefficient of -.006 on domestic partnerships for all couples represents a one percent drop in the likelihood of being married. This provides suggestive evidence that the stock of married people falls after opposite-sex couples can enter into non-marriage legal recognition but, as stated before, this number may be biased upward if people report domestic partnerships as marriages.

3.5 Conclusion

There has been much debate about what allowing same-sex couples to marry will do to the institution of marriage. This paper considers several possible avenues for how the legal changes that occurred during the first decade of the twenty-first century could have affected marriage. I find that allowing same-sex couples to marry increases overall marriage rates and that the effect on marriage rates is highest for the first few years after same-sex couples are allowed to marry. This increase is accounted for entirely by same-sex couples marrying. I find no effect of allowing same-sex couples to marry on opposite-sex marriage rates, which suggests that allowing same-sex couples the right to marry does not affect the value of marriage for opposite-sex couples. This is inconsistent with the end-of-marriage argument.

The evidence does suggest, however, that allowing opposite-sex couples to enter into new forms of legal recognition decreases marriage rates by about 10%. This means in the absence of domestic partnerships, many opposite-sex couples may enter into marriage even though they would actually rather enter into non-marriage legal recognition. Strengthening these domestic partnerships may make opposite-sex couples better off on average; however, strengthening the partnerships would also likely induce more people to enter into the partnerships instead of marriage.

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